

Professionals or Politicians: The Uncertain Empirical Case for an Elected Rather than Appointed Judiciary

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Conventional wisdom holds that appointed judges are superior to elected judges because appointed judges are less vulnerable to political pressure. However, there is little empirical evidence for this view. Using a data set of state high court opinions, we construct measures for three aspects of judicial performance: effort, skill, and independence. The measures permit a test of the relationship between performance and the four primary methods of state high court judge selection: partisan election, non-partisan election, merit plan, and appointment. Appointed judges write higher quality opinions than elected judges do, but elected judges write more opinions, and the evidence suggests that the large quantity difference makes up for the small quality difference. In addition, elected judges are not less independent than appointed judges. The results suggest that elected judges focus on providing service to the voters, whereas appointed judges care more about their long-term legacy as creators of precedent.

If the state has a problem with judicial impartiality, it is largely one the state brought upon itself by continuing the practice of popularly electing judges.

Justice O'Connor, concurring in *Republican Party of Minn. v. White*, 536 U.S. 765, 792 (2002).

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1. Introduction

Justice O'Connor's backhanded put-down of Minnesota's elected judiciary reflects the conventional wisdom among lawyers and scholars that judges should be appointed by elected officials or independent commissions and should not be elected themselves (Geyh 2003; Tarr 2003). The conventional wisdom reflects a deeply rooted conviction that voters are too unsophisticated to evaluate judges and candidates for judicial office. When judges use campaign contributions to finance simple-minded television commercials, conflict of interest is layered on public confusion. However, this conviction is hardly self-evident. In a system that uses judicial appointments, nothing forces the appointing official to select judges on the basis of their legal ability; cronyism is common. And, as the literature on voting shows, ordinary people use various strategies for evaluating candidates whose qualifications they do not fully understand. For example, they rely on party endorsements and newspaper editorials, and the give-and-take of the campaign.¹ And when many people participate in a decision-making process, aggregation of information occurs, which can produce more accurate results than when the decision is made by only one person.

The relative merits of appointment and electoral systems are an empirical question, but what exactly should be tested? Empirical work on this topic focuses on judicial independence, the willingness of a judge to vote against the ideological interests of the party of the elected official who appointed her or of the party to which she belongs. However, independence captures only a part of the judge's role. Judges are supposed to be independent but not arbitrary: a judge who votes against her party may still make bad decisions. And an independent judge who is lazy will not resolve many cases or will resolve them poorly. The measures of independence that have been used in the literature imply that the best judicial system would be one in which Democratic judges voted in favor of Republican interests and Republican judges voted in favor of Democratic interests. It is as though empirical studies of central banks focused exclusively on whether central banks made decisions that contradicted the expressed desires of the government and not on whether their decisions were correct, as a matter of monetary policy. Central bank independence is important but a central bank that always decided the opposite of what the government sought would not necessarily be a good one. The same can be said about judges.

To test the conventional wisdom that appointed judges are better than elected judges, we use a tripartite definition of judicial quality—productivity, opinion-quality, and independence. Productivity refers to the number of opinions a judge writes in a particular time period such as a year. The more opinions a judge writes, the more disputes she has resolved—and dispute resolution is the chief function of the judge. Opinion-quality refers to the opinion's reasoning. Better-reasoned opinions explain to the parties why they won or lost, but more important, they provide guidance to future judges who face similar cases,

1. Newspaper endorsements of judicial candidates are common.

and to people and businesses who want to avoid litigation in the first place. And independence refers to the willingness of judges to follow the law rather than the interests of political parties.

The independent variable of interest is the selection method for high court judges. In 12 states, judges are appointed by governors (or, in few instances, legislatures). At the opposite extreme, judges in nine states run for election—and reelection—as members of political parties. In between, there are two systems that combine partisan and nonpartisan elements. In 16 states, merit commissions are used: typically, an independent commission provides nominees whom the governor may appoint, whereas a retention election is used at the end of a judge's term (rather than a competitive election). In 13 states, nonpartisan elections are held: the public votes but judges are not permitted to advertise themselves as members of particular political parties.

A small empirical literature has investigated the relationship between selection systems and judicial characteristics in the states. As noted, the literature has focused on judicial independence and not other attributes of quality (Cann 2007; McLeod 2007; Shepherd 2007) and suggests that appointed judges are more independent than elected judges. Our tests of independence produce more complicated results and do not favor either system in a clear way. As for overall quality, again the literature assumes that appointed judges are better—albeit, with little attempt to measure quality (Cann 2007).² We find that elected judges are more productive. And although appointed judges write opinions that are cited more often, the difference is small and outweighed by the productivity difference. In other words, in a given time period, the product of the number of opinions authored and citations-per-opinion is higher for elected judges than for appointed judges.

After discussing our results, we attempt an explanation for why elected judges might differ in these ways from appointed judges. Elected judges look like politicians, whereas appointed judges look like professionals. Professionals care about their reputation among a national community of like-minded professionals, whereas politicians care about their reputation in the local community of lay voters and politicians. Appointed judges thus labor to write opinions that will be admired, whereas elected judges try to satisfy as many litigants as possible by dispensing quick but adequate justice. Our evidence does not prove that elected judges are superior to appointed judges, but it casts doubt on the conventional wisdom.

2. There are a handful of empirical studies that do attempt to construct quality measures so as to evaluate judicial performance. These studies use surveys, measures of educational qualifications, or measures of experience as their dependent variables (Canon 1972; Glick and Emmert 1987; Cann 2007). Surveys may reflect the biases of the respondents as we discuss in the text. As for educational and professional qualifications, those look to be more appropriate as an independent variable rather than as a measures of judging quality. (As we discuss below, there are interesting differences overlooked in the literature: elected judges went to worse law schools but have stronger local ties than do appointed judges).

2. Theory

2.1 The Determinants of Judicial Quality

Beginning with the legal realists, scholars and even judges themselves have speculated about the motives of judges, and whether judges decide cases by applying the law in a neutral fashion or in a manner that reflects personal or political views, or both (e.g., Newman 1984; Cross 2005). Because American judges have the power to strike down laws, the early controversy about judges' motives led to a debate about the proper role of judges in the constitutional system. If judges are ideologically motivated, then their power to strike down laws sits uneasily with democratic commitments; if they are not, or if their ideological motives are constrained, then judicial review has many attractive properties, including possibly the ability to prevent majorities from exploiting minorities or otherwise supporting bad law.³

In recent years, this debate has reemerged in the framework of an agency model, which treats the judiciary or individual judges as agents, and the public or particular elected officials as the principals (e.g., Maskin and Tirole 2004). Agency models warn that agents, unless properly selected, monitored, and rewarded, will not act in the interests of principals. In the context of the judiciary, political institutions need to be designed to ensure that people with preferences similar to that of the public are selected to be judges and that judges be given the right incentives to decide cases according to the law.

With respect to selection, judges should be impartial and competent, rather than partisan, ideological, eccentric, or incompetent. What selection mechanism will ensure that good, rather than bad, people are selected to be judges and that they will remain good after they become judges? Elections might ensure that people with mainstream views become judges, but the electorate may not be able to evaluate a potential judge's legal ability and other technical qualifications, let alone whether she is misleading the electorate in terms of her future fidelity to what is best for the populace. Appointments might result in competent and politically mainstream judges; however, elected officials might prefer to use judgeships as patronage positions. Unhappiness with these two extremes has led, in many states, to reliance on commissions, combinations of appointments and elections, and other mechanisms.

With respect to incentives, judges should, in principle, face sanctions if they decide cases poorly and rewards if they decide cases well. However, if rewards and sanctions are used, someone must apply them, and if that person has political power, judges might be afraid to decide cases impartially. The federal system avoids this risk by giving judges lifetime tenure on good behavior, but the danger with such a system is that it allows judges to decide cases badly or in a partisan fashion, without fear of sanction. Most state systems attempt to constrain judges by forcing them to undergo reelection or a reappointment process,

3. The vast literature cannot be described here. Holmes (1988) traces the history of this debate. Bickel (1986) is the source of the modern debate in constitutional law. Croley (1995) brings the debate to bear on state courts.

but the danger is that judges will decide cases in partisan fashion so as to avoid a partisan sanction.

The empirical literature on judicial behavior has focused primarily on federal judges, and especially on the US Supreme Court (George 1998). One vein of this literature establishes that judges' voting behavior reflects partisan or ideological preferences, at least to some extent. Judges who are Republicans or who are supported, at time of appointment, by conservative media, tend to vote in an ideologically conservative way; a corresponding bias characterizes Democrats (Segal and Spaeth 2002). This work confirms that unconstrained judges do not necessarily decide cases impartially and casts doubt on the assumption that appointment systems are necessarily best. Another vein in the literature shows that structural and institutional features influence and constrain judges' incentives to vote once on the bench. For example, perhaps because of whistleblower or group polarization effects, federal judges vote less ideologically when the panel is split by political party than when it is not (Cross and Tiller 1998; Sunstein et al. 2004).

A smaller literature on the state courts has come to similar results. Brace and various coauthors find correlations between voting and ideology, analogous to the federal court studies (e.g., Brace et al. 2001; Brace et al. 2006). Tabarrok and Helland (1999) find that tort awards are higher in electoral states than in nonelectoral states. They argue that their results reflect the stronger incentives of elected judges to redistribute wealth from out-of-state corporations to in-state voters and to please the local trial bar (see also Helland and Tabarrok 2002). Hanssen (1999) finds more litigation in nonelectoral states than in electoral states. He argues that high court judges in nonelectoral states have more independence and therefore are under less pressure to decide cases consistently. Greater uncertainty about the law generates more litigation. Hanssen (2000) finds that state bureaucracies are larger in nonelectoral states than in electoral states, which he attributes to defensive efforts by the agencies to protect themselves from less politically constrained judges. Pinello (1995) finds that appointed judges are more likely to favor criminal defendants than are elected judges. Besley and Payne (2006) find that employment discrimination claims are more numerous in electoral states than in nonelectoral states, which they argue shows that elected judges are more likely to rule in favor of employees than are appointed judges. Berkowitz and Clay (2006) find that the quality of state courts, as measured by surveys of senior attorneys at wealthy companies, is positively correlated with non-partisan judicial retention procedures. Shepherd (2007) focuses on the political party of "retention agents"—those people who decide whether a judge will be retained or not—and finds that judges (of whatever party) are more likely to vote in favor of traditional Republican interests when retention agents are Republicans and are more likely to vote in favor of traditional Democratic interests when retention agents are Democrats. The effect is larger when the retention process is electoral than when it involves reappointment. An early study by Landes and Posner (1980) finds that citations (including federal and out-of-state citations) of state supreme court opinions are uncorrelated with the selection system.

The literature, taken as a whole, provides evidence that selection and retention institutions influence judicial outcomes—by influencing who becomes a judge, or how judges decide cases, or both. The literature also confirms that judges are influenced by political factors. The literature largely skirts our topic—whether elected judges are, overall, better or worse than appointed judges—but offers tantalizing hints. The study of Tabarrok and Helland (1999) implies that elected judges are better agents of their constituents than are appointed judges; the social problem they identify is due to the federal structure of the country and the overlapping jurisdictions of states. Hanssen's (1999) finding that electoral pressures force state judges to be more consistent implies that elected judges are better—more consistent opinions, all else equal, are better than inconsistent opinions. Berkowitz and Clay (2006) do use a measure of overall judicial quality based on the views of senior attorneys at wealthy companies, but, as they acknowledge (pp. 412–13), the views of these senior attorneys are hardly unbiased. The study of Landes and Posner (1980) is closest to ours, but they do not look at productivity and independence, and their study has several methodological limitations.⁴ Our results are quite different from theirs.

Following the literature, we envision the relationship between the public and the judiciary as an agency relationship. The optimal selection mechanism minimizes agency costs. Judges expend unobservable effort to decide cases. Agency costs can take two forms: laziness (resulting in bad opinions or few opinions) and ideological self-indulgence (biased opinions). The optimal selection mechanism screens out judges with a strong preference for laziness or ideological self-indulgence and/or punishes judges who are observably lazy or ideologically self-indulgent.

Which type of selection mechanism is more likely to perform this function? The advantage of electoral systems is that, in principle, the public can select judges who appear energetic and politically neutral, and it can vote out of office judges whose judicial activity reflects laziness and political bias. The appointments system adds an extra decision-maker layer (typically the state governor), possibly dampening the public's ability to monitor judges. If appointed judges perform badly, the public would need to vote out of office the governor responsible for appointing the judges and reappointing bad judges, but the public would need to take account of other aspects of the governor's performance as well. On the other hand, if governors benefit from a well-functioning judicial system (because the business constituency is happy, for example) and are better able to monitor judges than is the public (because the governor and his or her staff are more sophisticated), then appointed judges might perform better than elected judges. So theory does not clearly point in either direction.

4. We use a larger sample, different variable definitions, and many more control variables. The Landes and Posner (1980) study is mainly about the federal courts. And their data come from a different era, before the modern concern about excessive political competition among state court justices fueled by campaign donations (Carrington 1998).

We do propose a more specific hypothesis, however. It seems reasonable to assume that in more populous states, the more dispersed public (the principal) would have more difficulty monitoring judges (the agents). The reason is as the number of monitors increases, the incentive to free ride on monitoring (which is a collective good) increases. By contrast, governors in larger states would not have more difficulty monitoring judges (though the public's ability to punish governors for failing to appoint and monitor judges effectively might be blunted). Thus, we predict that elected judges perform better in small states than in large states, whereas there is no or less difference in the performance of appointed judges in large and small states.

2.1 Judicial Quality and Judicial Selection Mechanisms

2.1.1 Judicial Quality. We use three measures of judicial quality.

Productivity: Judges have some discretion over how many opinions they write. Judges who write slowly will write fewer opinions. Judges who are lazy and wish to avoid writing concurrences and dissents, will also write fewer opinions. We measure a judge's productivity using the total number of opinions she writes during our sample time period (1998–2000), including majority, concurring, and dissenting opinions.

Productivity is a measure of effort, but is also a measure of quality because, all else equal, more opinions settle a greater number of legal disputes and resolve more legal issues. However, not all else is equal. A judge who writes more opinions might devote less time to each opinion, so that quality suffers. So productivity is at best only a partial measure of quality.

Citations: We assume, consistently with the rapidly expanding literature on judicial citations, that citations are a measure of quality (e.g., Landes et al. 1998; Cross and Lindquist 2006; Lott 2005; Baker 2007). Better opinions are cited more frequently than worse opinions. An opinion is cited frequently because it resolves a legal question or identifies some new legal problem or represents an advance in the law or simply clarifies doctrine. We focus on out-of-state citations because this allows us to control for the possibility that in-state citations reflect local legal customs or conventions. To check for robustness, we further subdivide citations into federal, state, and law review citations.⁵

Independence: Judges have the duty to enforce the law impartially, without regard to the legally irrelevant characteristics of the litigants or the goals of political parties. We thus assume that better judges are more independent. Some studies (e.g., Shepherd 2007) measure independence by the propensity of a judge to vote against interests associated with her party—for example, a Republican judge voting in favor of an employee and against an employer. This measure of independence focuses exclusively on the vote in the case and

5. The problems with citations studies have been rehearsed elsewhere and we will not repeat them here (e.g., on the possibility of bias, see Bhattacharya and Smyth 2004; Abramowicz and Tiller 2005; Choi and Gulati 2007).

does not take into account the direction of the opinion's reasoning. It would, for example, code an opinion that decides a particular case against an employee but creates precedent through its reasoning that would assist later employee suits, as Republican. And then there is the question of whether coding a vote against an employee and for the employer, regardless of the facts of the case, gets at the Republican/Democrat distinction at all. To avoid the foregoing pitfalls, we look directly at when judges decide to write opinions against judges of the same or opposite party. We posit that a judge who writes several dissents against majority opinions authored by judges of the same party (or majority opinions against dissents of same party judges) is more independent than a judge who rarely dissents and never against a judge of the same party.

Our measure of independence is imperfect, and we address its problems and alternatives subsequently. For now, we note two problems. First, the measure can be distorted by personal animosities. Personal animosities might cause judges to refuse to join opinions as often as they otherwise would (Choi and Gulati 2004). Second, there might be judges who are excessively partisan. These judges, because they view their co-partisan colleagues as not partisan enough, may end up dissenting a lot against the moderate judges from their own party. Such behavior—extremely partisan behavior—may then be interpreted as independence under our measure.

2.1.2 Selection Mechanisms. State judicial selection mechanisms can be divided into several ways. The literature has not arrived at a consistent methodology, and our approach differs from those of other researchers. Nonetheless, the approaches are roughly consistent, and we test ours for robustness. We divide judicial selection mechanisms into four categories (Table 1).

Governor or Legislative Appointment: In 12 states, judges are appointed by the governor or (in South Carolina and Virginia) the legislature. Gubernatorial appointments usually require the consent of the upper house of the legislature or the participation of a special commission such as an executive council. In most of these states, judges serve a term (ranging from 6 to 14 years) and then may be reappointed in the same manner. In Massachusetts, New Hampshire, and Rhode Island, judges enjoy lifetime or near-lifetime tenure.

Merit Plan: In 16 states, judges are nominated by a non-partisan commission and then appointed by the governor. Judges serve a term and then are subject to a retention election, where they run alone, and voters can either approve another term or vote against them. Terms vary but on the whole are less than those in appointment states.

Non-partisan Election: In 13 states, judges run for election. Their political affiliations are not listed on the ballot, and so voters, unless specifically informed, do not know a candidate's political party. These judges serve a term and then may run for reelection. The terms range from 6 to 10 years.

Partisan Election: In nine states, judges run for election as a member of a political party. They serve a term in the range of 6–10 years for the most part and then may run for reelection.

Table 1. Selection Systems

Appointed	Merit Selection	Non-Partisan Election	Partisan Election
Connecticut	Alaska	Georgia ^a	Alabama ^a
Delaware	Arizona ^a	Idaho	Arkansas
Hawaii	Colorado ^a	Kentucky ^a	Illinois ^a
Massachusetts ^a	Iowa	Louisiana ^a	North Carolina ^a
Maine	Indiana ^a	Michigan ^a	New Mexico
New Hampshire	Kansas	Minnesota ^a	Pennsylvania ^a
New Jersey ^a	Maryland ^a	Mississippi	Texas ^a
New York ^a	Missouri ^a	Montana	West Virginia
Rhode Island	Nebraska	North Dakota	
Vermont	Oklahoma	Nevada	
South Carolina	South Dakota	Ohio ^a	
Virginia ^a	Utah	Oregon	
	Wyoming	Washington ^a	
	California ^a	Wisconsin ^a	
	Florida ^a		
	Tennessee ^a		

^aIndicates that the state is in the top half of our sample states ranked by population in 1997.

Readers might be skeptical about whether voters care much about judicial elections and use the elections as an opportunity to reward good judges and punish bad judges. Hall (2001) finds that only 8.3% of state supreme court judges seeking reelection between 1980 and 1994 were defeated. Hall, nonetheless, reports a great deal of variation across time and selection systems. In partisan elections, judges during this period were defeated 18.8% of the time; in 1994, 36% of them were defeated. And judges' electoral success appears to hinge on their ideological similarity to voters. "The fact of the matter . . . is that supreme court justices face competition, that is, by two of three measures, equivalent if not higher to that for the US House" (Hall 2001: 319). (For further discussion, see Dudley 1997; Aspin 1999; Geyh 2003).

We should note that each state has a unique system; the categorizations suppress a great deal of variation. For example, the governor of Massachusetts appoints nominees of a judicial selection commission, whereas the governor of Maine makes appointments subject to confirmation by the Senate. Massachusetts judges serve until the age of 70; Maine judges have 7 year terms, at the end of which they may be reappointed by the governor, again subject to Senate confirmation. These differences have led to different coding practices in the literature, with some authors focusing on retention (e.g., Shepherd 2007) and others on selection procedures (e.g., Besley and Payne 2006); some using only two categories, others using multiple categories, and so forth.

Our categorization is similar to that of the other authors. The main concern is that if selection is relatively non-partisan and retention is relatively partisan, our selection variable will be misleading. Fortunately, it appears that the degree of partisanship tends to be the same at selection and retention decisions,

and indeed tenure length is negatively correlated with the partisanship of the selection process (see Table 3, below).

2.2 Hypothesis

Conventional wisdom is that appointed judges are better than elected judges. If so, appointed judges should have higher productivity, citation numbers, and independence. The existing empirical literature provides some support for the hypothesis that appointed judges are more independent. Our analysis adds an analysis of productivity and citation numbers in addition to a new measure of independence to get at the underlying question of whether appointed judges are of higher quality than elected judges.

3. Data Description

3.1 The Data Set

We examine the decisions of all the judges of the highest court of every state for the years 1998, 1999, and 2000. The District of Columbia is excluded because of its anomalous position. Two states—Texas and Oklahoma—have two highest courts, one with jurisdiction over civil appeals and the other with jurisdiction over criminal appeals. We, in effect, treat these courts as separate state courts: so we have 52 “states.”

Each court has a certain number of seats, but we count judges, rather than seats, so if turnover occurs, a court will have more judges than seats, and, if some seats are left empty, there could be fewer judges than seats. Our data set contains 408 judges, about eight per court. The average judge spent 2.65 of the 3 years in our sample period on the court. And each judge wrote on average about 67 opinions over his or her time in office.

We organize the data in three ways. For productivity, we run judge-year level regressions. Each observation is a judge for a particular year; there are 1082 observations—that is, the product of 408 and 2.65. For citations, we run opinion-level regressions to allow us to assess the factors that lead to citations to specific majority opinions. There are 27,596 opinion observations in our data set—consisting of 19,499 majority opinions (70.7%), 5669 dissenting opinions (20.5%), and 2428 concurring opinions (8.8%). For independence, we run judge-level regressions on data pooled from 1998 to 2000, and hence there are 408 observations. We assume that a judge’s independence does not change over the 1998–2000 time period and use pooled data to obtain as large a sample as possible of opposing opinions with which to construct our independence measure. In many of our regressions, the actual number of observations is lower as a result of inadequate data for variables of interest.

3.2 Measures of Judicial Quality

3.2.1 Productivity. Productivity is measured by total number of opinions written for any given year, including dissents and concurrences (Total

Opinions). Our least productive judge wrote two opinions in one year,⁶ whereas our most productive judge wrote 83 opinions in one year. The mean was 25.6 opinions per year. Table 2 provides productivity data arranged by type of selection system.

3.2.2 Opinion Quality. Our primary quality variable is the number of out-of-state citations to a particular opinion by a particular judge (Outside State Citations). We also look at narrower measures, such as law review citations and outside federal court citations.⁷

The best measure of quality is citation by an outside court—including another state court or outside federal court. Inside state and home federal court citations are driven to large extent by precedent. Looking at only outside citations allows us to examine those citations where judges have greater discretion to pick which opinions to cite. Judges are citing these outside opinions because they are helpful, not because they have precedential force. Because of this discretion, an outside circuit citation represents a better indication of which opinions judges deem of higher quality. Table 2 provides citation data, categorized by selection system.

3.2.3 Independence. Our independence measure focuses on the tendency of judges to write opinions that disagree with co-partisans when the pool of judges provides opportunities to do so.⁸ We define an “opposing opinion” as either a majority opinion when a dissent exists or a dissent when a majority exists. We assume that a judge exhibits independence when she writes an opposing opinion against a co-partisan.

We obtain a measure of the political party for each judge in our sample. We looked to three sources of information on party membership. First, we searched NEXIS and the Internet (using Google) for any news reports on the political affiliation of each judge. Second, we searched for information on political contributions at the *opensecrets.org* website.⁹ We used the

6. Probably because the judge left office early in the year, entered office late in the year, was sick during the year, or had administrative duties.

7. As a check, we also examine in-state and home federal circuit citations. We discuss the results of this robustness test later in the Article.

8. The variable is defined as follows: $indep = (demopratio - opdistratio) * (republican == 1) + (repopratio - opdistratio) * (democrat == 1)$, where *demopratio* is the fraction of majority opinions in the state written by a Democrat (and similarly for *repopratio*) and *opdistratio* is the fraction of opposing opinions written by the judge in question against a Democrat.

9. In the *Opensecrets* database, we searched for political contributions for each judge by first and last name in the state in which the judge sits on the high court. We also looked at the profession of each donor as provided by *Opensecrets*—counting only donations by persons with the same first and last name and who either listed their profession as on the state high court or who listed a law firm affiliation (where we were able to match the judge to the law firm through other sources).

Table 2. Measures of Judicial Performance

	Election Partisan	Election Non-Partisan	Merit	Appointed
Productivity measure				
Total Opinions	28.6	29.6	23.6	20.9 ^a
Majority Opinions	17.6	19.3	18.0	16.7
Dissenting Opinions	6.9	7.7	3.7	3.0 ^a
Quality measure				
Outside Federal Court	0.122	0.096	0.134	0.211 ^a
Other State Court	0.505	0.552	0.631	0.657 ^a
US Supreme Court	0.006	0.006	0.009	0.006
Outside State Citations	0.632	0.653	0.774	0.872 ^a
Law Review Citations	2.016	1.692	1.84	2.273
Independence measure				
Independence	-0.057	-0.025	-0.030	-0.028
Independence_Indicator	0.455	0.354	0.433	0.435

The productivity measure is the average number of opinions per judge per year in the designated category. Total Opinions includes majority, dissenting, and concurring opinions. The quality measure is the average number of citations per opinion. Outside Federal Court includes all citations from a federal district or circuit court located in a circuit that does not contain the state in question. Other State Court includes all citations from state courts outside of the state in question. US Supreme Court includes all citations from the US Supreme Court. Outside State Citations is the sum of Outside Federal Court + Other State Court + US Supreme Court. All citations are from the LEXIS Shepard's database and are tracked up until January 1, 2007. Law Review Citations are for law reviews as tracked by the LEXIS Shepard's database (until January 1, 2007). Independence is defined as the $\text{Opposite_Pool} - \text{Opposite_Party}$. Opposite_Party is the number of opposing opinions written against a judge of the opposite party divided by the number of opposing opinions written against a judge of either the opposite or same party from 1998 to 2000. Opposite Pool is the total number of majority opinions authored by an opposite party judge divided by the total number of majority opinions authored by either an opposite or same party judge from 1998 to 2000. $\text{Independence_Indicator}$ is defined as 1 if Independence is greater or equal to zero and 0 otherwise. Only judges for whom we could identify a political party were included in the analysis. We exclude judges from states where all judges in our sample were of the same political party from the analysis (Georgia, Maryland, New Mexico, South Carolina, South Dakota).

^aThe *t*-test of difference in means for Election Partisan and Appointed Judge is significant at the <1% level.

political party of the donee candidate as a proxy for the political party of judges who contributed. Where a judge contributed to candidates from more than one political party, we did not use the Opensecrets data to assign a political affiliation to the judge. Third, we used the party of the governor (if any) who appointed the judge as a proxy for the judge's political party. In most of the cases where we had multiple sources of information on political party, the party was consistent across these sources. Where we found no data on the judge's political affiliation or the judge's affiliation was neither a Democrat nor a Republican (but was instead an Independent), we ignored the judge for purposes of calculating the Independence measure. When our three sources reported different parties, we gave first priority to the party identified through our NEXIS and Internet searches, second to the party identified in the opensecrets.org database, and third to the party of the appointing governor. In our sample, 220 judges were classified as a Democrat and 170 as a Republican (with 16 no data or Independent party judges). Of the 390 judges classified as

either a Democrat or Republican, 35 (or 8.97%) had a conflict in our three methods of determining political affiliation.

We define *Opposite_Party* as the number of opposing opinions written, by the judge of interest, against a judge of the opposite party divided by the number of opposing opinions written against a judge of either party from 1998 to 2000. This variable measures propensity to side with co-partisans. Not all opposing opinions are driven by the ideology of the opposing judges. A judge who dissents at random would dissent 70% of the time against an opposite party judge if the background pool of majority opinions consisted of 70% opposite party authored opinions. To take into account the background pool of opinions, we define *Opposite Pool* as the total number of majority opinions authored by an opposite party judge divided by the total number of majority opinions authored by either an opposite or same party judge (not including the judge in question) from 1998 to 2000.

We define *Independence* as *Opposite_Pool* minus *Opposite_Party*. A more negative *Independence* score corresponds to a judge who writes opposing opinions against opposite party judges more frequently than the background pool of majority opinions authored by opposite party judges. Conversely, a more positive *Independence* score corresponds to an authoring judge who writes opposing opinions less frequently against opposite party judges compared with the background pool of opinions (and thus more frequently against co-partisans). We treat a more positive *Independence* score as indicative of a more independent judge.¹⁰

Table 2 reports summary statistics on our *Independence* measure. Only judges for whom we could identify a political party were included in the analysis, and only judges from states that had a mixture of judges from different political parties were included.¹¹

Two problems may affect our *Independence* measure. First, consider the extreme case where all judges on a particular state high court are all of the same political party (say all Republican). In this case, our *Independence* measure will equal zero since *Opposite_Party* will equal *Opposite_Pool* (and both will equal zero since there are no Democrat-authored opinions). Our *Independence* measure in Table 2 excludes judges who come from states with no

10. Consider, for example, a Republican judge sitting on a high court in a state where the other judges are split 50–50 between Republican and Democrat judges and the pool of majority opinions written by other judges corresponds to this 50–50 split. Suppose our Republican judge authors 10 dissents and 20 majority opinions where there is a dissenting opinion. And suppose that 5 of the 10 dissents are authored against a Democrat judge and 15 of the 20 majority opinions face a dissent from a Democrat judge. In this case, *Opposite_Party* would equal $(5+15)/(10+20) = 2/3$. *Opposite_Pool* equals $1/2$. *Independence* would then equal $1/2 - 2/3 = -1/6$. Because of the tendency of our Republican judge to write an opposing opinion more frequently against Democrats compared with the background pool of majority opinions, the Republican judge receives a negative *Independence* score.

11. States where all judges in our sample were of the same political party included Georgia, Maryland, New Mexico, South Carolina, and South Dakota.

variation in political party among judges for this reason. But, by the same token, we lose data.

Second, even where all judges are not of the same political party in a state, if an imbalance exists, the range of the Independence variable will vary. Consider two Republican judges. One is in a state with 90% of the majority opinions written by Democrats and the other is in a state with 10% of the majority opinions written by Democrats. For the first, Independence can range from -0.1 to $+0.9$. For the second, Independence can range from -0.9 to $+0.1$. So the second judge could have a much more negative Independence score than the first judge simply because the range is shifted over.

To address these problems, we create a version of the Independence variable that is less dependent on the political makeup of a court. Independence_Indicator is defined as 1 if Independence is greater or equal to zero and 0 otherwise. The indicator variable addresses the range problem but also throws out information: it suggests judges subject to nonpartisan elections are less independent than the other types, who are about the same. The last row of the Independence panel of Table 2 reports on the mean of Independence_Indicator. None of the differences in mean Independence_Indicator levels among the varying selection systems are statistically significant.

In our multivariate tests, we also consider the possibility that the act of writing a dissenting opinion (even against opposite-partisan judges) can be a greater display of independence than writing a majority opinion (even against co-partisan dissenters). Judges who write dissents display independence in their willingness to write critically of the opinions of their colleagues.

4. Multivariate Tests

We estimate a number of multivariate regression models to assess the relationship of several key independent variables, including the judicial selection mechanism, and our measures of judicial quality (used as the dependent variables in our models). We also include a number of common control variables in our models.

4.1 Selection Mechanisms

Our main interest is the relationship between the selection mechanism used by states to select and retain judges and our dependent variable measures of judicial productivity, quality, and independence. Table 1 describes the selection systems for the different states. In all the states, the selection mechanism long predates our data pool and thus mitigates concerns about endogeneity—that states adopted new mechanisms in response to changes in judicial quality. Electoral systems can be traced back to the Jacksonian era. The switch to alternative systems generally occurred during the Progressive Era, and the

process was more or less complete by the 1970s. Recent changes, with the exception of South Carolina, have been marginal (Hanssen 1999; Besley and Payne 2006, table 1).

To check for robustness, we test the importance of judicial tenure data—the average tenure (meaning actual service, not *de jure* term length) of high court judges by state, as of the spring of 1997 (from Hanssen 1999, table 1) (Tenure). The Tenure data does not come from our data set: we do not have information on the tenure of the judges, many of whom are still in office. We provide summary statistics on the relationship between Tenure and the different judge selection mechanisms in Table 3.

Note from Table 3 that mean tenure rises as the involvement of the public in the selection of judges falls. We have two clusters—election systems and low tenure, and appointment/merit plan systems and high tenure.

The advantage of the Tenure variable is that it allows us to compare the different selection mechanisms along a common metric: the length of time that a judge expects to remain in office. Only a few appointment states have lifetime tenure, so even in systems where the public lacks the power to select judges, judges may be “punished”: elected officials or commissions unhappy with the performance of judges can refuse to reappoint them or support their reappointment. Thus, tenure indicates the vulnerability of the judge to later retention decisions more directly than do the selection mechanisms. Judges with longer tenure face less retention pressure, whereas judges with shorter tenure face greater retention pressure. Tenure allows us to measure the real effects of the retention mechanisms while allowing us to rely less on the *de jure* rules that might be evaded in local legal practice.

However, problems exist with the Tenure variable. If a judge’s performance displeases the retention decision maker, the judge may be deprived of an additional term, in which case she ends up with a short tenure. Judges in equilibrium may respond to the threat of a frequent retention decisions by catering to the preferences of the retention decision maker. Thus, a judge with a long tenure could represent either a judge who rarely faces a retention decision or, alternatively, a judge who does face frequent retention decisions but who in equilibrium is adept at satisfying the preferences of the

Table 3. Tenure Data

	Number of states	Mean	Standard deviation	Minimum	Maximum
Partisan Election	9	6.7	2.4	3.5	11.1
Non-Partisan Election	14	7.1	1.5	4.6	9.1
Merit Selection	17	10.0	4.5	4.0	19.1
Appointed	12	9.2	3.1	5.0	14.9
Total	52	8.5	3.4	3.5	19.1

Tenure is defined as the average tenure of high court judges for the state in question, measured as of the spring of 1997 (from Hanssen 1999, table 1).

retention decision maker. The strong correlation between Tenure and state selection systems that provide for longer (if not lifetime) tenure leads us to discount this latter possibility. But because of these potential problems with Tenure, we use Tenure only as a robustness check of our selection system results.

4.2 Control Variables

Our multivariate models of judicial productivity, quality, and independence include a common set of control variables. First, the multivariate models include judge level controls (referred to as “Judge Controls”).

We include an indicator variable for whether the judge was the chief judge of the high court (Chief Judge). A judge who is chief judge may have less time to author opinions. The chief judge may also command greater respect and receive greater numbers of citations as a result for her opinions. Alternatively, the chief may be able to assign herself the more important opinions and garner more citations that way (Langer 2003). We include the number of years between 1998 and the year in which the judge received her law degree (Post Law-School Experience) and the number of years the judge has been on the high court (Court Experience). More experienced judges may decide opinions with greater skill, leading to more citations. We include variables for whether a judge retired within 1 year or less (Retirement within 1 year) or in exactly 2 years, 3 years, or 4 years (Retirement in 2 Years; Retirement in 3 years; Retirement in 4 years)—judges who retire soon have little to lose from deciding badly.

We also include a number of variables specific to the background of the individual judge. These include the age of the judge (Age), an indicator variable for the whether the judge is female (Female), and an indicator variable for whether the judge’s primary experience before becoming a judge was in private practice (Private Practice). We include an indicator variable for whether the judge raised funds relating to election campaign expenditures for the current year (Election Spending). Lastly, we include the PAJID score for each judge as developed by Brace et al. (2000). These scores locate judges on a political continuum from highly conservative (0) to highly liberal (100).

It is possible that the selection system may filter out and select for specific types of judges. Including judge-level controls separate from our selection system variables may therefore understate the effect of the selection system variables. On the other hand, the judge-specific variables may have an effect independent from the selection system. For example, those selecting judges may only care marginally about a judge’s age. However, the age of a judge may nonetheless directly affect productivity and our other measures of judicial performance. Including age separately allows us to account for this separate influence. We alternatively include and exclude our judge-level controls in our multivariate models.

Second, we include court level controls that attempt to capture differences among the state high courts that might account for judicial performance (referred to as “Court Controls”). We include measures for the average high court associate justice salary (Adjusted Associate Justice Salary) and the average partner salary in the state (Adjusted Partner Salary). The salary variables are adjusted for the cost of living for the metro area in which the high court is located in the state. Higher paying states may attract higher quality judges. Perhaps judges work harder if their salary will decline (or not rise much) if they are not retained. We include an indicator variable for whether the judges on the high court remained the same throughout our sample time period from 1998 to 2000 (Stable Court) and the size of the bench during the 1998–2000 period (Number of Active Judges on Bench). We include an indicator variable for whether the judges in a specific court do not face mandatory retirement (No Mandatory Retirement).

As a measure of resources available to high court judges, we include the average number of clerks per judge for the 1998–2000 period (Number of Clerks Per Judge) and an indicator variable for whether the clerks are tenured for at least 1 year (Long-Term Clerk). To capture the opportunity cost of being a law clerk, the difference between the average salary of an entering associate at law firm in that state and the law clerk salary is used (Law Clerk Opportunity Cost). Judges may also act differently if facing a high workload, particularly if an intermediate appellate level court does not exist to help with the workload. We include the log of the number of trial cases in the state measured in 1998 ($\ln(\text{Number of Trial Cases in the State})$) and an indicator variable for the presence of an intermediate appellate court (Intermediate Appellate Court). Specific court rules may affect the workload facing judges, thereby affecting the level of judicial output. We include an indicator variable for whether judges face a mandatory publication rule (Mandatory Publication). The variables for the number of clerks, clerk tenure, clerk opportunity cost, the size of the bench, the number of trial cases, and the existence of an intermediate appellate court may all influence a judge’s choice to devote time to any specific case.¹²

Finally, to mediate the effects of state-level differences on our results, we include variables relating to the characteristics of each state (referred to as “State Controls”). Differences in overall state population

12. We also construct indicator variables for whether the state high court has mandatory jurisdiction over civil (Mandatory Civil Jurisdiction) or criminal (Mandatory Criminal Jurisdiction) cases. We also total the number of petitions filed with the state high court (Total Cases Filed) and the number of petitions where the high court granted a hearing (Total Cases Granted). Both sets of variables relate to the workload facing a judge. Unfortunately, we lack information on these variables for all of our states. We therefore do not include them in our set of state control variables but instead use them for robustness tests as discussed later in the paper.

($\ln(\text{Population})$), gross state product ($\ln(\text{Gross State Product})$), and crime rates (Crime Index) may lead to different mixes of cases and judicial responses to these cases. Likewise, the median age of the population (Median Age of Population) and state median per capita income (State Median Income) as measured in the 2000 US Census may affect the mix of cases and judicial response. Because previous research suggests that state judges are influenced by judges in neighboring states (Harris 1985) and because larger neighboring states might produce different types of cases, we include a variable for the aggregate population of border states ($\ln(\text{Border Population})$). We also include a measure of the age of the state (State Age). Older states have longer judicial traditions and hence possibly a more sophisticated jurisprudence on which modern judges can draw. State Age controls for the possibility that modern judges are cited more often outside of the state just because they can draw on the older and more sophisticated jurisprudence of their particular state. We include the fraction of the population comprised of blacks as obtained from the 2000 Census (Black Population Fraction). Greater racial heterogeneity may produce greater complexity in the mix of cases that go to the state high court and affect a judge's attitudes toward such cases. The background ideology of the citizens of a state may affect the preferences of high court judges.

Table 4 contains summary statistics for the control variables.

4.3 Productivity

Our first multivariate model focuses on judicial productivity. We estimate the following equation on judge year-level data using an ordinary least squares regression with robust standard errors clustered by state:

$$\begin{aligned} \ln(1 + \text{Total_Opinions})_i = & \alpha + \beta_{1i} \text{Election_Partisan} \\ & + \beta_{2i} \text{Election_Nonpartisan} + \beta_{3i} \text{Merit_Plan} \\ & + \sum \beta_{ji} \text{Judge Controls}_{ji} \\ & + \sum \beta_{ki} \text{Court Controls}_{ki} \\ & + \sum \beta_{li} \text{State Controls}_{li} + \text{Year Effects} + \varepsilon_i. \end{aligned}$$

Our model relates the log of the total number of opinions authored by a judge in any given year to three indicator variables for Partisan Election, Non-Partisan Election, and Merit Plan states. The three indicator variables use Appointed states as the baseline. The regression model includes year-level Judge, Court, and State Controls described above (see Appendix for definitions). We introduce the three sets of control variables separately (Models 1–3) and together (Model 4) to assess the effect of the controls on the relationship between the selection system and judicial output of opinions. The model also includes year fixed effects. Table 5 reports results.

Table 4. Summary Statistics

	Election partisan	Election non-partisan	Merit	Appointed
Judge controls				
Adjusted Associate Justice Salary (1000s)	112.661	104.014	107.789	98.085 ^a
Adjusted Partner Salary (1000s)	226.086	200.874	213.275	183.316 ^a
Chief Judge	0.145	0.141	0.173	0.160
Court Experience	6.595	7.523	9.435	8.454 ^a
Post-Law School Experience	29.820	30.054	31.830	32.773 ^a
Retirement within ≤ 1 year	0.250	0.137	0.122	0.181 ^c
Retirement in 2 years	0.095	0.056	0.057	0.076
Retirement in 3 years	0.055	0.056	0.063	0.059
Retirement in 4 years	0.055	0.065	0.066	0.067
Age	57.090	57.337	58.839	59.378 ^a
Female	0.205	0.307	0.190	0.298 ^b
Private Practice	0.840	0.866	0.857	0.735 ^a
Election Spending	0.115	0.147	0.077	0.000 ^a
PAJID Score	34.192	37.871	32.192	47.849 ^a
Summary statistics reported for judge-year level data. See definitions in the Appendix.				
Court controls				
Stable Court	0.111	0.286	0.235	0.333
Number of Active Judges	8.889	8.214	7.294	7.250 ^c
No Mandatory Retirement	0.222	0.286	0.294	0.250
Long-Term Clerk	0.556	0.571	0.563	0.250
Number of Clerks Per Judge	2.365	2.178	2.059	1.958
Law Clerk Opportunity Cost	34.721	23.871	28.232	34.004
Trial Cases in the State (1000s)	4106.699	1391.621	1650.835	1612.924 ^c
Intermediate Appellate Court	0.889	0.786	0.882	0.583
Mandatory Publication	0.222	0.214	0.235	0.083
Total Cases Filed	3208.125	1506.923	1651.438	1670.167
Total Cases Granted	563.143	556.400	532.909	570.400
Mandatory Civil Jurisdiction	0.250	0.357	0.313	0.583
Mandatory Criminal Jurisdiction	0.000	0.357	0.313	0.583 ^a
Summary statistics reported for state level data. See definitions in the Appendix.				
State controls				
State Age	163.111	152.643	131.765	192.833
State Population (mill.)	8.956	4.468	5.572	4.332 ^c
Population in Border States (millions)	28.718	23.289	22.243	18.164 ^c
Crime Index	4889.908	4773.046	5104.385	4087.724
Median Age of Population	35.322	35.264	34.888	36.658 ^c
Gross State Product (billions)	295.829	145.766	178.841	163.272
State Median Income (1000s)	26.073	26.874	27.702	31.942 ^b
Black Pop. Fraction	0.129	0.109	0.073	0.100
Citizen Ideology Score	47.477	43.234	38.946	61.398 ^a
Summary statistics reported for state level data. See definitions in the Appendix.				

^aThe *t*-test of difference in means for Election Partisan and Appointed Judge is significant at the <1% level.^bThe *t*-test of difference in means for Election Partisan and Appointed Judge is significant at the 5% level.^cThe *t*-test of difference in means for Election Partisan and Appointed Judge is significant at the 10% level.

The results in Table 5 contradict the hypothesis that judges subject to more partisan pressure are less productive; the opposite is the case. The coefficients on Election Partisan are positive and significant from the 10% to <1% levels in three of the four models. The results are less strong for Election Non-Partisan and Merit judges. In the models with only Judge (Model 1) and Court Controls (Model 2), the coefficients on Election Non-Partisan and Merit are positive but not significantly different from zero. In the models with State Controls alone (Model 3) and State Controls together with Judge and Court Controls (Model 4), the coefficients on Election Partisan, Election Non-Partisan and Merit are all positive and significant. In Model 4 (with all control variables), Partisan-elected judges are the most productive, followed by merit plan and nonpartisan judges. Appointed judges (the omitted, baseline case) are the least productive.¹³

13. To assess whether any particular subset of the state controls is important in affecting the relationship between the judge selection system and productivity (a concern highlighted by the large coefficients for state population and gross state product, which are larger than the coefficients for the election variables), we ran several variations of Model 4 of Table 5 alternatively using the following subsets of our state controls: (1) State Age, (2) $\ln(\text{State Population})$ and $\ln(\text{Border Population})$, (3) Crime Index and Median Age of Population, (4) $\ln(\text{Gross State Product})$ and State Median Income, (5) Black Pop. Fraction, and Citizen Ideology Score. The coefficient for Election Partisan is positive and significant ranging from the <1% to 10% levels for each variation except for the variation using the $\ln(\text{Gross State Product})$ and State Median Income variables (where the coefficient for Election Partisan is positive but insignificant). In contrast, the coefficient for Election Non-Partisan is positive and significant only for the variation with state age. The coefficient for Merit Plan is not significantly different from zero in all of the variations. As with the models of Table 5, these variations are generally consistent with the result that Election Partisan judges are more productive than Appointed judges.

To further test the importance of state size, we replaced $\ln(\text{Population})$ in Model 4 of Table 5 with several alternate measures related to state size (in separate regressions) including the number of law enforcement agencies, the number of full-time law enforcement employees, the number of prosecutions, the number of law-related employees, the annual payroll for all law-related employees, and the number of law establishments for a particular state (all measured in 2000). The coefficients for Election Partisan, Election Non-Partisan, and Merit Plan are positive and significant (at either the 5% or <1% level) for each regression. The coefficient for each of our alternate measures of state size is positive, but significant only for the number of full-time law enforcement employees, the number of law-related employees, the annual payroll for all law-related employees, and the number of law establishments.

We ran a series of placebo regressions to test whether state-specific unobservables may affect the relationship of our judge selection system variables and productivity. We randomized the observations in Model 4 into four equally sized groupings (to parallel the four selection systems in Model 4). We replaced Election Partisan, Election Non-Partisan, and Merit Plan with three of the groupings, using the fourth as the base category. We then estimated this randomized groupings model 100 times. The coefficient on the randomized Election Partisan variable was significant at the 5% level a total of 8 times (and at the 10% level a total of 15 times) out of 100 times. The coefficient on the randomized Election Non-Partisan variable was significant at the 5% level a total of 9 times (and at the 10% level a total of 11 times) out of 100 times. The coefficient on the randomized Election Non-Partisan variable was significant at the 5% level a total of 11 times (and at the 10% level a total of 18 times) out of 100 times. The relative infrequency of significant results from the randomized groupings coupled with the high level of significance of the results of Model 4 of Table 5 lead us to view Model 4 as correctly specified and the significant results for our judge selection variables in Model 4 as robust.

Table 5. Productivity

Independent variables	Model 1	Model 2	Model 3	Model 4
Election Partisan	0.284 (1.46)	0.346 ⁺ (1.87)	0.511* (2.32)	0.616** (3.67)
Election Nonpartisan	0.252 (1.49)	0.170 (0.97)	0.533** (3.26)	0.291* (2.03)
Merit Plan	0.091 (0.72)	0.087 (0.58)	0.581** (3.09)	0.284 ⁺ (1.76)
Chief Judge	-0.176** (-3.29)			-0.146** (-2.80)
Court Experience	0.015* (2.65)			0.012** (2.72)
Post-Law School Experience	0.004 (0.56)			0.007 (1.39)
Retirement within ≤1 year	-0.280** (-3.10)			-0.304** (-4.37)
Retirement in 2 years	-0.113 (-1.08)			-0.105 (-1.42)
Retirement in 3 years	-0.110 (-1.18)			-0.099 (-1.55)
Retirement in 4 years	-0.168* (-2.01)			-0.072 (-1.00)
Age	-0.001 (-0.20)			-0.001 (-0.20)
Female	-0.091 (-1.18)			-0.032 (-0.54)
Private Practice	-0.014 (-0.17)			-0.053 (-0.63)
Election Spending	-0.038 (-0.59)			-0.021 (-0.39)
PAJID Score	0.001 (0.97)			0.003 ⁺ (-1.80)
Adjusted Associate Justice Salary		0.009* (2.23)		0.007 (1.64)
Adjusted Partner Salary		-0.001 (-0.99)		-0.001 (-0.88)
Stable Court		0.366* (2.32)		0.331* (2.31)
Number of Active Judges		0.011 (0.26)		-0.030 (-1.05)
No Mandatory Retirement		-0.307 ⁺ (-1.88)		-0.249 ⁺ (-1.98)
Long-Term Clerk		-0.144 (-1.04)		-0.053 (-0.50)
Number of Clerks Per Judge		0.093 (0.97)		0.042 (0.53)
Law Clerk Opportunity Cost		-0.005 (-1.10)		-0.009* (-2.56)
ln(Trial Cases in the State)		-0.197* (-2.42)		-0.132 ⁺ (-1.89)
Intermediate Appellate Court		0.046		0.053

Continued

Table 5. *Continued*

Independent variables	Model 1	Model 2	Model 3	Model 4
		(0.20)		(0.24)
Mandatory Publication		0.134		0.205 ⁺
		(0.75)		(1.82)
State Age			0.004*	0.004*
			(2.06)	(2.16)
ln(State Population)			0.976 ⁺	1.934**
			(1.96)	(3.50)
ln(Population in Border States)			-0.211**	-0.278**
			(-3.64)	(-4.84)
Crime Index			0.000*	0.000
			(-2.62)	(-1.54)
Median Age of Population			-0.012	-0.035
			(-0.33)	(-0.95)
ln(Gross State Product)			-1.049 ⁺	-1.921**
			(-1.93)	(-3.43)
State Median Income			0.020	0.054 ⁺
			(0.70)	(1.97)
Black Pop. Fraction			1.287 ⁺	0.367
			(1.94)	(0.51)
Citizen Ideology Score			0.001	-0.004
			(0.28)	(-0.64)
Constant	2.889**	3.529**	6.714**	10.518**
	(10.38)	(7.30)	(3.44)	(4.60)
Year Fixed Effects	Yes	Yes	Yes	Yes
N	1019	1053	1074	998
Adjusted R ²	0.0465	0.1072	0.1658	0.2578

Dependent variable is $\ln(1 + \text{Total Opinions})$. The *t*-statistics (in parentheses) are calculated using robust standard errors clustered by state. Variable definitions are in the Appendix.

⁺Coefficient significant at the 10% level or less.

*Coefficient significant at the 5% level or less.

**Coefficient significant at less than the 1% level.

Our control variables largely make sense and are interesting in their own right. More highly paid judges are more productive, taking into account the opportunity cost of forgoing a practicing lawyer's salary—though the magnitude is small. Judges on a stable court are more productive, probably because collegial norms are stronger on stable courts. Chief judges are less productive, perhaps because they have administrative duties. Judges with more experience on the court are more productive; but judges become less productive as they approach retirement (particularly in the last year before retirement). Judges from states with a large population are more productive because they have more cases to decide, and, because of the greater activity and diversity of the population, more law to make (but with statistical significance in only one of two models). The coefficients on the presence of an

intermediate appellate court and the crime index variables are either statistically insignificant or small in magnitude.¹⁴

Why are judges subject to electoral pressures more productive? Perhaps productivity is used by voters as a signal of judicial competence, or by intermediaries, such as newspaper editorialists, bar associations, and parties. Judges who write few opinions, then, will be vulnerable in reelection campaigns.¹⁵ This would also explain why productivity declines as

14. As a robustness test, we reestimated Model 4 of Table 5 with the addition, alternatively, of (1) the Total Opinions Filed with the high court, (2) the Total Opinions Granted a hearing before the high court, and (3) indicator variables for whether the high court has mandatory civil jurisdiction and mandatory criminal jurisdiction. We lack data on these variables for all our states. Nonetheless, unreported, we obtained largely the same qualitative results as in Model 4 in the three robustness models. Judges from Election Non-Partisan, Election Partisan, and Merit Plan states are more productive than Appointed judges. In the model with the addition of the Total Opinions Granted a hearing before the high court as an independent variable, however, the coefficient on Election Nonpartisan while positive is significant at only the 11% level.

We also reestimated Model 4 using the log of 1 + the total number of majority opinions in any given year and, alternatively, the log of 1 + the total pages written for all opinions in any given year as dependent variables. Unreported, we obtained largely the same qualitative results as in Model 4 in both robustness models. Judges from Election Non-Partisan, Election Partisan, and Merit Plan states are more productive than Appointed judges (whether measured by total majority opinions or total pages written). In the majority opinion model, however, the coefficient on Election Non-Partisan while positive is not statistically significant.

To test the individual importance of each year in our data set, we reestimated Model 4 separately for 1998, 1999, and 2000. In each model, the coefficient on Partisan Election is positive, significant, and greater in magnitude compared with the positive coefficients on Partisan Non-Election, and Merit Plan—consistent with the results in Model 4. The coefficients on Partisan Election and Partisan Non-Election are positive and significant at the 5% and 10% levels, respectively, while the coefficient on Merit Plan is insignificant for the 1998 model. Only the coefficient on Partisan Election is significant (at the 5% level and positive) for the 1999 model. The coefficients on Partisan Election, Partisan Non-Election, and Merit Plan are all positive and significant (ranging from the <1% to 5% level) for the 2000 model.

Lastly, we reestimated Model 4 with the addition of indicator control variables for whether the state is a member of US Census region Midwest, Northeast, or South (using West as the base case). Because substantial collinearity exists between our judge selection system variables and our census region variables (e.g., almost all judges from the Northeast Census region are Appointed judges), we add census region variables as a robustness test only. Unreported, the coefficient on Election Partisan, Election Non-Partisan, and Merit Plan are all positive and significant at the <1% level, consistent with the results in Table 5.

15. Anecdotal evidence supports the hypothesis that elected judges feel pressure to be productive. Productivity is mentioned in newspaper endorsements and judicial evaluation materials from time to time. A clear example can be found in the reelection campaign materials of a Texas intermediate appellate court judge, which include a table with productivity statistics for a group of judges:

For the fiscal year ending August 31, 2006, Third Court of Appeals Justice Bob Pemberton ranked #1 statewide among Texas' court of appeals judges in production of original appellate opinions on the merits. These results show that Justice Pemberton is the most productive appeals judge in Texas for original opinions and the Third Court of Appeals is the most productive of Texas' 14 courts of appeals.

http://www.bobpemberton.com/2006/09/20/appeals_opinion_productivity/.

Table 6. Variations on Productivity Model

	Model 1	Model 2	Model 3	Model 4
Dependent variable	ln(1+Total Opinions)	ln(1+Total Opinions in Low Salience Areas)	ln(1+Total Opinions in High Salience Areas)	ln(1+Total Opinions)
Independent variables				
Election Partisan	1.219** (4.930)	0.600** (2.890)	0.665** (3.780)	
Election Non-Partisan	0.738** (3.180)	0.332* (2.110)	0.099 (0.510)	
Merit Plan	0.651* (2.240)	0.331+ (1.770)	0.059 (0.310)	
Election Partisan × Large State	-0.800* (-2.600)			
Election Non-Partisan × Large State	-0.601+ (-2.000)			
Merit Plan × Large State	-0.461 (-1.100)			
Large State	0.181 (0.540)			
Tenure				-0.032+ (-1.930)
Constant	8.975** (3.850)	10.059** (3.730)	9.310** (3.250)	10.124** (4.640)
Judge Controls	Yes	Yes	Yes	Yes
Court Controls	Yes	Yes	Yes	Yes
State Controls	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
<i>N</i>	998	994	934	998
Adjusted R^2	0.2848	0.2575	0.1922	0.2416

The *t*-statistics (in parentheses) are calculated using robust standard errors clustered by state. Variable definitions are in the Appendix.

+Coefficient significant at the 10% level or less.

*Coefficient significant at the 5% level or less.

**Coefficient significant at less than the 1% level.

retirement nears, as reported in both Models 2 (Judge Controls only) and 4 (with all control variables).

If elected judges respond to electoral pressures through increased productivity, one might predict that productivity may vary with the size of the state. The public in large states has more trouble monitoring judges; hence these judges would be less productive. To test the effect of state size, we start with Model 4 of Table 5 and include an indicator variable for whether the state is in the top half of states in terms of population size (Large State). We also include interaction terms between Large State and Election Partisan, Election Non-Partisan, and Merit Plan. Model 1 of Table 6 below reports our results.

Model 1 of Table 6 reports that the coefficient on Large State is not significantly different from zero. In contrast, the coefficients on the interaction terms between Large State and Election Partisan and Election Non-Partisan are negative and significant at the 5% and 10% levels, respectively. The sum of Large State + Election Non-Partisan \times Large State is negative and significant at the 10% level; the sum of Large State + Election Partisan \times Large State is negative and significant at the 5% level. Although judges from Election Non-Partisan and Election Partisan states are generally more productive, evidence exists that the amount of increased productivity diminishes in states with a large population—particularly for Election Partisan judges (but still remains positive compared with judges from Appointed states).¹⁶

Production of published opinions may also vary with the public salience of the subject matter of the opinion. For hot button issues, judges may have more (or less) inclination to produce a published opinion. Judges in Election states, for example, may wish to shy away from publishing in controversial areas (such as capital punishment) to avoid angering constituents. On the other hand, judges eager to make a name for themselves may gravitate to publishing in more salient areas. To test the importance of public salience, we again start with Model 4 of Table 5 and estimate the model separately using as the dependent variable, alternatively, the log of the total number of opinions dealing with Low Salience and High Salience subject matter areas.

As detailed in the Appendix, we classify opinions into 12 subject categories. The high salience categories are taken from Choi and Gulati (2008) and include Administrative Law, Capital Punishment, Church and State, First Amendment, and Property Rights cases. Following Epstein and Segal (2000), Choi and Gulati count news stories relating to the US Supreme Court on the front page of the New York Times to determine which subject categories are of public salience. Although the methodology uses the US Supreme Court, we assume that the general public will find the salient subject matters similarly high profile in the state court context. Models 2 and 3 of Table 6 report results.

Judges in Election Partisan, Election Non-Partisan, and Merit Plan states are all more productive compared with Appointed judges for low salience opinions. However, only Election Partisan judges are more productive than Appointed Judges for high salience opinions. Election Partisan judges perhaps use high salience opinions to signal their political credentials to voters in the state. Appointed Judges and Election Non-Partisan judges do not have similar incentives.

We next replace the three indicator variables for the state judge selection system with Tenure, our alternative measure of the pressure facing judges, focusing on the retention decision. Model 4 of Table 6 reports that the coefficient on Tenure is negative and significant at the 10% level. Judges who face

16. The sum of Election Partisan + Large State + Election Partisan \times Large State is positive and significant at the 5.3% level. The sum of Election Nonpartisan + Large State + Election Nonpartisan \times Large State is positive and insignificant however.

frequent retention decisions (and presumably experience a shorter expected tenure) are more productive than judges who have the luxury of a longer expected tenure. These results are consistent with the results of the other productivity models above.

4.4 Citations

For outside citations, we estimate the following equation for each majority opinion using ordinary least squares and robust standard errors clustered by judge:

$$\begin{aligned} \ln(1 + \text{Outside State Citations}_i) &= \alpha + \beta_{1i}\text{Election_Partisan} + \beta_{2i}\text{Election_Nonpartisan} \\ &+ \beta_{3i}\text{Merit_Plan} + \beta_{4i}\text{Number of Dissents} + \beta_{5i}\text{West Key Pages} \\ &+ \beta_{6i}\text{Opinion Length} + \sum \beta_{ji}\text{Subject Matter}_{ji} \\ &+ \sum \beta_{ki}\text{Judge Controls}_{ki} + \sum \beta_{li}\text{Court Controls}_{li} \\ &+ \sum \beta_{mi}\text{State Controls}_{mi} + \text{Year Effects} + \varepsilon_i. \end{aligned}$$

The model relates the number of outside state citations (Outside State Citations) for any specific majority opinion with three indicator variables for Election Partisan, Election Non-Partisan, and Merit Plan states. The three indicator variables use Appointed states as the baseline. The model includes the number of dissents written against the majority opinion in question (Number of Dissents). A majority opinion with one or more dissents may deal with more novel issues of law and generate more citations as a result. The model includes the number of west key pages (West Key Pages) as a rough measure of the legal importance of the opinion. Similarly the model includes the length of the opinion (Opinion Length); longer opinions are more likely to contain analysis that other judges may cite compared with shorter opinions, all other things being equal. We also include subject matter fixed effects for the 12 different subject matter categories, including Administrative, Attorney and Client, Capital Punishment, Church and State, Commercial, Criminal, Family, First Amendment, Labor, Property, Rights, and Torts (see Appendix for definitions). We use Other opinions as the baseline subject matter category.

We include Judge, Court, and State Controls, as described earlier. We introduce the three set of control variables separately (Models 1–3) and together (Model 4) to assess the affect of the controls on the relationship between the selection system and judicial output of opinions. We also use year fixed effects. Table 7 reports the results from our multivariate outside citation model.

Our results are largely consistent with the hypothesis that judges subject to less partisan pressure write higher quality—more frequently cited—opinions. In the first three models, the coefficient on Election Partisan is negative and significant (ranging from the 5% to 10% level); the coefficient

Table 7. Citations

Independent variables	Model 1	Model 2	Model 3	Model 4
Election Partisan	-0.073* (-2.29)	-0.084* (-2.62)	-0.076+ (-1.95)	-0.040 (-1.34)
Election Non-Partisan	-0.069 (-1.67)	-0.008 (-0.21)	-0.050 (-1.28)	0.008 (0.28)
Merit Plan	-0.038 (-1.23)	-0.023 (-0.73)	-0.075 (-1.66)	-0.056 (-1.66)
Number of Dissents	0.078** (4.88)	0.065** (5.99)	0.070** (6.15)	0.060** (6.34)
West Key Pages	0.021* (2.53)	0.032** (3.40)	0.015* (2.11)	0.023** (3.26)
Opinion Length	0.032** (11.26)	0.032** (11.54)	0.033** (12.65)	0.032** (11.60)
Constant	0.056 (0.76)	-0.171 (-1.55)	-1.646** (-3.12)	-2.390** (-5.77)
Subject Matter Categories	Yes	Yes	Yes	Yes
Judge Controls	Yes	No	No	Yes
Court Controls	No	Yes	No	Yes
State Controls	No	No	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
<i>N</i>	18,623	19,159	19,461	18,321
<i>R</i> ²	0.1249	0.1257	0.1386	0.1341

Dependent variable is $\ln(1+\text{Outside Citations})$. The *t*-statistics (in parentheses) are calculated using robust standard errors clustered by state. Variable definitions are in the Appendix.*Coefficient significant at the 10% level or less.

*Coefficient significant at the 5% level or less.

**Coefficient significant at less than the 1% level.

on Election Partisan is negative but insignificant in Model 4 however. The coefficients on Election Non-Partisan and Merit Plan are negative but not significantly different from zero across all of the models. Overall, and particularly compared with Election Partisan judges, the models are consistent with the view that Appointed judges write the best opinions. Also, majority opinions with dissents are cited more frequently, perhaps because the presence of a dissent indicates that the majority opinion makes new law. This reason might also explain why longer opinions are cited more often.¹⁷

17. As a robustness test, we also reestimated Model 4 of Table 7 with the addition, alternatively, of (1) the Total Opinions Filed with the high court, (2) the Total Opinions Granted a hearing before the high court, and (3) indicator variables for whether the high court has mandatory civil jurisdiction and mandatory criminal jurisdiction. We lack data on these variables for all our states. Nonetheless, unreported, we obtained similar qualitative results as in the Models of Table 7. The coefficients on Election Nonpartisan, Election Partisan, and Merit Plan are negative in all the models. The coefficient for Election Partisan is generally (but not always) significant. The coefficient for Election Nonpartisan is insignificant. The coefficient for Merit Plan is significant (either at the 5% or 10% level). Judges from Election Partisan and Merit Plan states are generally cited less than Appointed judges in these variations of Model 4.

Why would appointed judges write better opinions than elected judges? One possibility is that the quality of an opinion, unlike the number of opinions written, is not observable to the public, so elected judges do not have a strong incentive to write high-quality opinions. With less pressure to produce, appointed judges might prefer to advance their influence and professional reputation by writing good opinions. Another possibility is that a system that selects for judges skilled at electioneering and politicking does not also necessarily select for judges skilled at authoring high-quality legal opinions.¹⁸

To test the importance of large states, we start with Model 4 of Table 7 and include the Large State indicator variable as well as interaction terms between Large State and Election Partisan, Election Non-Partisan, and Merit Plan. Model 1 of Table 8 reports the results.

18. As a robustness test, we reestimated Model 4 of Table 7 using the log of one plus the number of law review citations to a majority opinion ($\ln(1 + \text{Law Review Citations})$) as an alternative measure of opinion quality (and as the dependent variable in the model). Not reported, we obtain similar results as in the Models of Table 7. Judges from Election Partisan, Election Nonpartisan, and Merit Plan states produce opinions that are cited less by law reviews than judges from Appointed states. Unlike in Model 4, the coefficients for Election Partisan and Merit Plan are significant at the 5% level. The biggest drop off in the level of law review citations is for Election Partisan opinions.

We also reestimated Model 4 with the use of $\ln(1 + \text{Same State Citations} + \text{Home Federal Citations})$ as the dependent variable. Same State Citations include all in-state citations; Home Federal Citations include citations from a federal court in the same circuit as the state in question. Unreported, the coefficients on Election Partisan and Election Non-Partisan are not significantly different from zero. Only the coefficient on Merit Plan is significant (at the 5% level); however, the coefficient is positive, indicating that Merit Plan judges produce opinions that are cited more by same state and home federal cases compared with Appointed judges. Same state citations are not driven by the same factors behind out of state citations; same state citations, for example, often occur because of the need to cite to precedent.

To test the individual importance of each year in our data set, we reestimated Model 4 separately for 1998, 1999, and 2000. In all three year-specific models, the coefficients on Election Partisan and Election Nonpartisan are not significant. In two of the models (1998 and 2000), the coefficient on Merit Plan is negative and significant at the 5% and 10% levels.

We reestimated Model 4 with the addition of indicator control variables for whether the state is a member of US Census region Midwest, Northeast, or South (using West as the base case). Unreported, the coefficient on Election Partisan and Merit Plan are negative and significant at the 10% and 5% levels, respectively. The coefficient on Election Non-Partisan, although negative, however, is not significantly different from zero.

Lastly, we ran a series of placebo regressions to test whether state-specific unobservables may affect the relationship of our judge selection system variables and outside state citations. We randomized the observations in Model 4 of Table 7 into four equally sized groupings (to parallel the four selection systems in Model 4). We replaced Election Partisan, Election Nonpartisan, and Merit Plan with three of the groupings, using the fourth as the base category. We then estimated this randomized groupings model 100 times. The coefficient on the randomized Election Partisan variable was significant at the 5% level a total of 3 times (and at the 10% level a total of nine times) out of 100 times. The coefficient on the randomized Election Non-Partisan variable was significant at the 5% level a total of 6 times (and at the 10% level a total of eight times) out of 100 times. The coefficient on the randomized Election Non-Partisan variable was significant at the 5% level a total of five times (and at the 10% level a total of eight times) out of 100 times. The relative infrequency of significant results from the randomized groupings leads us to view Model 4 of Table 7 as correctly specified.

Table 8. Variations on Citation Model

	Model 1	Model 2	Model 3	Model 4
Dependent variable	ln(1+Outside Citations)	ln(1+Outside Citations) for Low Salience Opinions	ln(1+Outside Citations) for High Salience Opinions	ln(1+Outside Citations)
Independent variables				
Election Partisan	0.013 (0.26)	-0.070* (-2.15)	-0.029 (-0.65)	
Election Non-Partisan	-0.013 (-0.26)	-0.011 (-0.34)	0.010 (0.23)	
Merit Plan	-0.090 (-1.48)	-0.067+ (-1.74)	-0.045 (-1.19)	
Election Partisan × Large State	-0.103* (-2.04)			
Election Non-Partisan × Large State	-0.011 (-0.19)			
Merit Plan × Large State	0.035 (0.43)			
Large State	0.034 (0.54)			
Tenure				0.000 (-0.12)
Number of Dissents	0.061** (6.46)	0.063** (6.59)	0.056+ (1.98)	0.063** (6.43)
West Key Pages	0.023** (3.23)	0.009 (1.31)	0.069** (3.32)	0.023** (3.17)
Opinion Length	0.032** (11.72)	0.034** (11.02)	0.016** (5.03)	0.031** (11.99)
Constant	-2.279** (-5.04)	-2.611** (-5.89)	-0.197 (-0.27)	-2.041** (-4.61)
Subject Matter Categories	Yes	No	No	Yes
Judge Controls	Yes	Yes	Yes	Yes
Court Controls	Yes	Yes	Yes	Yes
State Controls	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
<i>N</i>	18,321	16,996	1325	18,321
Adjusted <i>R</i> ²	0.1374	0.1183	0.1216	0.1327

Dependent variable is ln(1+Outside Citations). The *t*-statistics (in parentheses) are calculated using robust standard errors clustered by judge. Variable definitions are in the Appendix.

+Coefficient significant at the 10% level or less.

*Coefficient significant at the 5% level or less.

**Coefficient significant at the 1% level.

The most dramatic difference in the model with Large State and the interaction terms with Large State is the result for the citations to Election Partisan judges. The coefficient on Election Partisan is positive and not significantly different from zero. Opinions authored by judges from smaller states with

partisan elections receive a similar number of citations to that of appointed judges. The relationship changes, however, for larger states. The coefficient on Election Partisan \times Large State is negative and significant at the 5% level. Moreover, the sum of Large State + Election Partisan \times Large State is negative and significantly different from zero at the 10% level. Outside state citations diminish for opinions written by Election Partisan judges in larger states compared with smaller states. Perhaps because judges in larger states cannot be as effectively monitored by electorates than judges in smaller states, their opinion quality is lower in larger states.

To test the importance of public salience on opinion quality, we start with Model 4 of Table 7 and estimate the model solely for opinions in a low salience subject matter area and solely for opinions in a high salience area. Model 2 (low salience) and Model 3 (high salience) of Table 8 reports results. Model 2 reports that the coefficients for Election Partisan and Merit Plan are negative and significant at the 5% and 10% levels, respectively, for low salience opinions; the coefficient on Election Non-Partisan is not significantly different from zero. In contrast, the coefficients on Election Partisan, Election Non-Partisan, and Merit Plan are not significantly different from zero in Model 3 for high salience opinions. Judges from Election Partisan and Merit Plan states write lower quality opinions primarily for low salience subject matter areas.

We lastly replace the three indicator variables for the state judge selection system with Tenure, our alternative measure of the pressure facing judges, focusing on the retention decision. Model 4 of Table 8 reports that the coefficient on Tenure is not significantly different from zero. Unlike our productivity model, we do not find evidence that judges that face more retention pressure differ from judges with less retention pressure in the quality of opinions produced.

4.5 Independence

4.5.1 Propensity to Dissent. To assess what factors correlate with a high propensity to write dissenting opinions, we estimate the following equation on judge-year level data using ordinary least squares and robust standard errors clustered by state:

$$\begin{aligned} \ln(1 + \text{Total Dissents})_i = & \alpha + \beta_{1i} \text{Election_Partisan} \\ & + \beta_{2i} \text{Election_Nonpartisan} + \beta_{3i} \text{Merit_Plan} \\ & + \sum \beta_{ji} \text{Judge Controls}_{ji} \\ & + \sum \beta_{ki} \text{Court Controls}_{ki} \\ & + \sum \beta_{li} \text{State Controls}_{li} + \text{Year Effects} + \varepsilon_i. \end{aligned}$$

The regression model relates the number of dissenting opinions written in a given year ($\ln(1 + \text{Total Dissents})$) with three indicator variables for Partisan Election, Non-Partisan Election, and Merit Plan states. The indicator variables use Appointed states as the baseline. The model includes year-level versions of the State Controls and Judge Controls as well as year fixed effects. Table 9 reports the model.

Table 9. Dissents

	Model 1	Model 2
Dependent variable	ln(1+Total Dissents)	ln(1+Total Dissents)
Independent variables		
Election Partisan	0.927** (3.90)	
Election Non-Partisan	0.769** (4.51)	
Merit Plan	0.244 (1.17)	
Tenure		-0.056** (-3.03)
Constant	4.005 (1.31)	4.942 (1.66)
Year Fixed Effects	Yes	Yes
Court Controls	Yes	Yes
State Controls	Yes	Yes
<i>N</i>	998	998
Adjusted <i>R</i> ²	0.2203	0.1835

Dependent variable is ln(1+Total Dissents). The *t*-statistics (in parentheses) are calculated using robust standard errors clustered by state. Variable definitions are in the Appendix.

†Coefficient significant at the 10% level or less.

*Coefficient significant at the 5% level or less.

**Coefficient significant at less than the 1% level.

Model 1 of Table 9 indicates that elected judges, especially partisan-elected judges, dissent the most, with merit-plan judges in the middle. Appointed judges write the fewest dissenting opinions.

Model 2 of Table 9 reports the results of our robustness test using Tenure as an alternative specification of how judges differ across the states with a focus on the retention decision. Note that the coefficient on Tenure in Model 2 is negative and significant at the <1% level. Judges who face the possibility of a short tenure (as in elected states) write more dissents.

If dissenting indicates a willingness to express one's honest view, then our results suggest that elected judges are more independent.¹⁹

4.5.2 Propensity to Write Opinions against Co-Partisans (Independence). Given the shortcomings of focusing solely on the number of dissenting opinions as a measure of independence, we turn to our Independence measure. We estimate the following equation on pooled data from 1998 to 2000 using an ordinary least squares regression model with robust standard errors clustered by state:

19. We also find a positive correlation between dissent activity and the size of the bench (i.e., the number of judges participating in cases during our period), which is consistent with Lindquist (2007), who finds the same result using federal appellate courts.

$$\begin{aligned}
 \text{Independence}_i = & \alpha + \beta_{1i}\text{Election_Partisan} + \beta_{2i}\text{Election_Nonpartisan} \\
 & + \beta_{3i}\text{Merit_Plan} + \sum \beta_{ji}\text{Subject Matter}_{ji} \\
 & + \sum \beta_{ki}\text{Judge Controls}_{li} + \sum \beta_{li}\text{Court Controls}_{ki} \\
 & + \sum \beta_{mi}\text{State Controls}_{li} + \varepsilon_i.
 \end{aligned}$$

The regression model relates our Independence measure (Independence) based on pooled 1998 to 2000 data for each judge with three indicator variables for Non-Partisan Election, Partisan Election, and Merit Plan states. The three variables use Appointed states as the baseline. As a control for the subject matter composition of the pool of opinions, for each of the 12 subject matter categories (see Appendix for definitions) we compute the number of majority opinions that deal with the specific subject matter divided by the total number of majority opinions for the state in the 1998 to 2000 time period. We include this ratio for each subject matter in the model as pooled controls for subject matter in the state. The model includes pooled data version of the Judge, Court, and State controls as described above. We introduce the three set of control variables separately (Models 1–3) and together (Model 4) to assess the affect of the controls on the relationship between the selection system and judicial output of opinions. Table 10 reports results.

In all four models of Table 10, the coefficient on Election Partisan is not significantly different from zero. Moreover the magnitude is small. Our Independence measure ranges from -0.804 to 0.737 for the sample judges. The

Table 10. Independence

Independent variables	Model 1	Model 2	Model 3	Model 4
Election Partisan	0.010 (0.29)	0.023 (0.64)	-0.075 (-1.16)	-0.043 (-0.59)
Election Non-Partisan	-0.044 (-1.34)	-0.060 ⁺ (-1.85)	-0.127 ⁺ (-1.85)	-0.150 (-1.60)
Merit Plan	-0.052 (-1.40)	-0.068* (-2.19)	-0.147* (-2.08)	-0.154 ⁺ (-1.70)
Constant	-0.197 (-0.40)	1.360* (2.34)	1.639 ⁺ (1.95)	1.882 (1.49)
Subject Matter Categories	Yes	Yes	Yes	Yes
Judge Controls	Yes	No	No	Yes
Court Controls	No	Yes	No	Yes
State Controls	No	No	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
N	331	345	352	324
Adjusted R ²	0.0878	0.0966	0.0529	0.1191

Dependent variable is Independence. The *t*-statistics (in parentheses) are calculated using robust standard errors clustered by state. We exclude judges from states where all judges in our sample were of the same political party from the analysis (Georgia, Maryland, New Mexico, South Carolina, South Dakota). Variable definitions are in the Appendix.⁺Coefficient significant at the 10% level or less.

*Coefficient significant at the 5% level or less.

**Coefficient significant at less than the 1% level.

coefficient for Election Partisan in Model 4 is equal to -0.043 , representing only 2.8% of the range between the sample minimum and maximum. To understand this figure, imagine that an elected judge and an appointed judge sit on benches that are otherwise identical in terms of partisan composition: the elected judge is 2.8 percentage points more likely to disagree with an opposite party judge than an appointed judge is. Table 10 suggests that judges from Election Partisan states are not any less (or more) independent than judges from Appointed states.

In contrast, the story is mixed for Election Non-Partisan and Merit Plan judges. In the Court control-only model (Model 2), the State control-only model (Model 3) and the model with Judge, Court, and State controls (Model 4), the coefficients on Election Non-Partisan and Merit Plan are negative and significant at levels ranging from 5% to 10% (in one case falling a bit short of the 10% level). Appointed judges appear more independent than judges from these other two types of states. The magnitude of the difference in Independence between Appointed and Election Non-Partisan and Merit Plan states is relatively large (compared with the difference between Appointed and Election Partisan judges). The coefficient for Election Non-Partisan is equal to -0.150 and the coefficient for Merit Plan is equal to -0.154 in Model 4, representing 9.7% and 10.0%, respectively, of the range between the sample minimum and maximum Independence scores.

We consider the possibility that the act of writing a dissenting opinion (even against opposite partisan judges) can be a greater display of independence than writing a majority opinion (even against co-partisan dissenters). Judges who write dissents display independence in their willingness to write critically of the opinions of their colleagues. To see if those who dissent more frequently are also more independent, we add the total number of dissents written in our sample period for each judge (Total Dissents) and interaction terms between Total Dissents and Election Partisan, Election Non-Partisan, and Merit Plan to Model 4 of Table 10. We report the results in Table 11.

Model 1 of Table 11 reports that the coefficients on Election Partisan, Election Non-Partisan, and Merit Plan are insignificant. The coefficient on Total Dissents is positive and significant at the $<1\%$ level. Those judges who write more dissents are also more independent under our Independence measure. In contrast, the coefficient on the interaction term between Election Partisan and Total Dissents is negative and significant at the 10% level. The sum of Total Dissents and Election Partisan \times Total Dissents is also not significantly different from zero. Although judges who write more dissenting opinions generally receive a higher Independence score (particularly for Appointed State judges), this relationship does not hold true for judges in Election Partisan states.

We posit that more ideologically intense judges will tend to act less independently when faced with the possibility of an election. To test this, we add an indicator variable for whether the judge contributed to a political candidate as tracked in the Opensecrets database (Opensecrets) as a proxy for political intensity to Model 4 of Table 10. We also add interaction terms between Open Secrets and the three indicator variables for the state selection systems.

Table 11. Variations on the Independence Model

	Model 1	Model 2	Model 3	Model 4
Dependent variable	Independence	Independence	Independence	Indep_ Indicator
Independent variables				
Election Partisan	-0.031 (-0.39)	-0.022 (-0.27)	0.256** (3.08)	-0.710 (-0.76)
Election Non-Partisan	-0.148 (-1.58)	-0.122 (-1.32)	0.026 (0.34)	-2.656** (-2.59)
Merit Plan	-0.151 (-1.59)	-0.167 ⁺ (-1.95)	0.104 (1.43)	-2.848* (-2.56)
Election Partisan × Total Dissents	-0.004 ⁺ (-1.85)			
Election Non-Partisan × Total Dissents	-0.003 (0.88)			
Merit Plan × Total Dissents	-0.002 (-0.88)			
Total Dissents	0.004** (5.08)			
Election Partisan × Opensecrets		-0.167 ⁺ (-2.01)		
Election Non-Partisan × Opensecrets		-0.194* (-2.15)		
Merit Plan × Opensecrets		-0.123 (-1.35)		
Opensecrets		0.087 (1.26)		
Election Partisan × Large State			-0.177* (-2.71)	
Election Non-Partisan × Large State			0.063 (1.16)	
Merit Plan × Large State			-0.103 (-1.39)	
Large State			-0.029 (-0.49)	
Constant	1.862 (1.33)	1.840 (1.48)	2.130 ⁺ (1.77)	44.035** (3.04)
Subject Matter Categories	Yes	Yes	Yes	Yes
Judge Controls	Yes	Yes	Yes	Yes
Court Controls	Yes	Yes	Yes	Yes
State Controls	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
N	324	324	324	309
Adjusted or Pseudo R ²	0.1133	0.1354	0.1314	0.1942

The *t*-statistics (in parentheses) are calculated using robust standard errors clustered by state. We exclude judges from states where all judges in our sample were of the same political party from the analysis (Georgia, Maryland, New Mexico, South Carolina, South Dakota). Variable definitions are in the Appendix.

⁺Coefficient significant at the 10% level or less.

*Coefficient significant at the 5% level or less.

**Coefficient significant at less than the 1% level.

Model 2 of Table 11 reports that the interaction terms between *Opensecrets* and *Election Partisan* and *Election Non-Partisan* are both negative (indicating less independence) and significant at the 10% and 5% levels, respectively. Although more intensely political judges do not correlate significantly with a decreased independence score for *Appointed* and *Merit Plan* judges, we see a negative relationship between political intensity and independence for the elected judges.

To test the importance of large states, we start with Model 4 of Table 10 and include the *Large State* indicator variable as well as interaction terms between *Large State* and *Election Partisan*, *Election Non-Partisan*, and *Merit Plan*. Model 3 of Table 11 reports the results. Now the coefficient on *Election Partisan* is positive and significant at the <1% level. For smaller states, judges from *Election Partisan* states are more independent than from *Appointed* states. This relationship changes however for larger states. The coefficient on *Election Partisan* \times *Large State* is negative and significant at the 5% level. Moreover, the sum of the coefficients on *Large State* + *Election Partisan* \times *Large State* is negative and significant at the <1% level. Judges from larger *Election Partisan* states are less independent than judges from smaller *Election Partisan* states. This result is consistent with our hypothesis that electorates in larger states might have more trouble monitoring judges than electorates in smaller states.²⁰ In contrast, the coefficient on *Large State* alone is not significantly different from zero. *Appointed* judges in larger states are not more (or less) independent than *appointed* judges in smaller states.

20. As discussed, our Independence measure suffers from a range problem. To address this, we reestimate Model 4 of Table 10 using *Indep_Indicator* as the dependent variable, set equal to 1 if Independence is greater or equal to 0 and set equal to 0 otherwise. The use of *Indep_Indicator* lessens the range problem but at the cost of less data. Model 4 of Table 11 reports the results. As with Model 4 of Table 10, the coefficient on *Election Partisan* is not significant while the coefficients on *Election Non-Partisan* and *Merit Plan* are negative and significant.

As a robustness test, we reestimate Model 4 of Table 10 with the addition of indicator control variables for whether the state is a member of US Census region Midwest, Northeast, or South (using West as the base case). In the robustness model, none of the coefficients on *Election Partisan*, *Election Non-Partisan*, and *Merit Plan* are significantly different from zero, indicating relatively little difference among the different selection systems in terms of judicial independence.

Lastly, we ran a series of placebo regressions to test whether state-specific unobservables may affect the relationship of our judge selection system variables and the Independence measure. We randomized the observations in Model 4 of Table 10 into four equally sized groupings (to parallel the four selection systems in Model 4). We replaced *Election Partisan*, *Election Non-Partisan*, and *Merit Plan* with three of the groupings, using the fourth as the base category. We then estimated this randomized groupings model 100 times. The coefficient on the randomized *Election Partisan* variable was significant at the 5% level a total of 3 times (and at the 10% level a total of four times) out of 100 times. The coefficient on the randomized *Election Non-Partisan* variable was significant at the 5% level a total of 7 times (and at the 10% level a total of 11 times) out of 100 times. The coefficient on the randomized *Election Non-Partisan* variable was significant at the 5% level a total of 5 times (and at the 10% level a total of seven times) out of 100 times. The relative infrequency of significant results from the randomized groupings leads us to view Model 4 of Table 10 as correctly specified.

Our results are complicated and difficult to summarize, but our overall sense is that elected judges are more likely to dissent (suggesting more independence), and the two types of judges are otherwise roughly equally like to write against co-partisans (suggesting equal independence). Strongly partisan judges act in a more partisan way in electoral systems than in appointment systems, but it is not clear why this matters if independence for overall judicial activity is the same. What is clear is that the conventional wisdom that appointed judges are more independent than elected judges is a simplification and probably an exaggeration.

4.6 Productivity versus Quality

Given the ambiguity of our independence results, what's more important: productivity or quality? As a measure of the overall influence of a judge's opinions, we calculated the aggregate number of outside citations to the opinions written by each judge in 1 year (Aggregate Outside Citations). The average judge in a Partisan Election system in 1 year writes opinions cited (outside of both the state and home federal circuit) in the aggregate 11.3 times, whereas the average judge in an Appointed system writes opinions cited in the aggregate 15.0 times (difference significant at the <1% level). The numbers for Non-Partisan Election systems and Merit selection systems are 12.6 and 14.0, respectively. At a summary statistic level, the influence of appointed judges is significantly greater than that of elected judges despite the lower productivity on the part of appointed judges.

To provide a multivariate test controlling for state and judge characteristics, we reestimate our judge productivity model (Model 4 of Table 5) on judge-year level data, replacing $\ln(1 + \text{Total Opinions})$ with $\ln(1 + \text{Aggregate Outside Citations})$ as the dependent variable. Unreported, the coefficient on Election Partisan is positive and significant at the <1% level, using Appointed as the base case. Judges from Election Partisan states have significantly higher levels of aggregate outside citations than do Appointed judges. The coefficients on Partisan Non-Election and Merit Plan are also positive and significant at the 5% level. In contrast with the summary statistic comparison, the multivariate test supports the view that the lack of productivity on the part of appointed judges diminishes the overall influence and quality of their total judicial output of opinions relative to Election Partisan judges. The differing results between the summary statistic comparison and the multivariate model at the very least call into question the conventional wisdom that appointed judges are superior to elected judges.

We reestimate the multivariate model adding the Large State indicator variable and interaction terms between Large State and the judge selection system variables. Unreported, the coefficient on Large State is positive and significant at the <1% level. The sum of Large State and Large State \times Partisan Election however is negative and significant at the <1% level.²¹ Large States have better overall judicial performance but this relationship reverses for Partisan

21. The sum of Large State and Large State \times Non-Partisan Election is positive and insignificant. The sum of Large State and Large State \times Merit Plan is negative and insignificant.

Election states: judges in larger Partisan Election states perform more poorly than judges in smaller Partisan Election states. The reduction in performance for judges in larger Election Partisan states is consistent with our hypothesis that the public is less able to monitor judges through elections in large states.

Our results highlight a problem with earlier studies that used only a single measure of judicial quality (such as judicial independence): if judicial quality is multidimensional, as it surely is, judges might choose to maximize along one dimension rather than another, depending on the incentives that they face. A study might show that “quality” is correlated with some institutional factor such as selection mechanism when, in fact, only a particular dimension of quality is correlated and overall quality is not. We return to this problem in Section 5.

5. Explaining the Results

We summarize our results in Table 12.

Elected judges write more opinions, but their average citation counts per opinion are lower than those for appointed judges. From the average litigant’s perspective, elected judges provide more justice to more people—if one takes the receipt of a written explanation for the court’s decision as a component of justice. Appointed judges write fewer opinions, but those that are written tend to be higher quality opinions—they garner more citations. Fewer litigants are receiving this high-quality justice though. Judges in the different systems exhibit similar levels of independence, with perhaps somewhat lower scores for Non-Partisan Election judges and Merit Plan judges. Elected judges in small states perform better on all quality measures than elected judges in large states; size of state does not make a difference for appointed judges. What explains these results?

We began with a simple agency model, which did not have clear predictions for whether elected or appointed judges would be superior, but did suggest that size of state would matter more for elected judges than for appointed judges, as our results confirm. We can get further insight from the multitasking model (Holmstrom and Milgrom 1991), which shows that if an agent is given two objectives (say, quality and productivity), and the activities that further only one of those objectives can be measured and monitored by the principal, then the agent will shirk on the hard-to-measure objective and invest in achieving the other.

Judges in more partisan systems may write more opinions because raw productivity is observable. Judges who write few opinions, for example, can be

Table 12. Selection Mechanisms and Quality

Selection Mechanism	Tenure	Productivity	Independence	Quality
Election Partisan	Lower	High	Higher	Low
Election Non-Partisan	Lower	Middle	Lower	Middle
Merit Plan	Higher	Middle	Lower	Low
Appointed	Higher	Low	Higher	High

easily criticized and, as we have seen, some judges use productivity as a campaign issue. And, as the size of the electorate increases, judges might feel less pressure to be productive because it is harder for the public to monitor judges' productivity. Quality is hard to observe. With respect to quality, the public may have more trouble monitoring judges than governors do; our citations results are consistent with this hypothesis. Also note that again, within electoral states, quality does matter in small states, and not in large states, consistent with the hypothesis that small electorates are better monitors than are larger electorates.

As for independence, the propensity to vote with or against co-partisans is also hard to observe. If so, elected judges would have no more incentive to refrain from acting independently than appointed judges do. So it is, in the end, not surprising that the independence levels of elected and appointed judges are not clearly different. Interestingly, elected judges are more independent in small states compared with large states. This again suggests that size of state matters: electorates may disapprove of politically biased judges but be unable to monitor them if the electorate is too large.

There is another interpretation of the data, one that focuses on selection rather than monitoring. It might be that the different systems attract different types of people to judgeships: selection matters more than incentives to behave once in office. In particular, electoral systems would seem to attract *politicians*, whereas appointment systems are more likely to attract *professionals*. Politicians want to satisfy the voting public, and this might mean deciding cases expeditiously and in great number. Professionals are more concerned about their reputation among other lawyers and judges and are more interested in delivering well-crafted opinions that these others will admire.

Recall from the summary statistics presented in Table 4 above that, compared with Appointed judges, Elected judges make more campaign contributions; are paid less; are on less stable benches; and have shorter tenures. We also examined the law school that our sample judges attended. A little under 70% of our Election Partisan judges attended an in-state law school compared with only 33% of the Appointed judges (difference significant at the <1% level). In addition, the US News Ranking (measured as of 2005) of the law school for Election Partisan judges was on average equal to 57.9, whereas the mean ranking for Appointed judges was equal to 32.3 (difference significant at the <1% level). Election Partisan judges are more likely to have gone to a law school in the state in which they sit and are more likely to have gone to a lower-rank law school. They are, in short, more politically involved, more locally connected, more temporary, and less well educated than appointed judges. They are more like politicians and less like professionals. Politicians are likely to see their role as judges as predominantly one of resolving disputes (as many as possible), whereas professionals are more likely to see their role as advancing the law. This might explain the productivity and quality differences.

One might think that politicians would be more concerned about party identification, and hence would be more likely to vote together than professionals

are—but this is not the case, as our independence measures show. One simple explanation for our results is that for most cases, one's vote has no political salience. The vast majority of cases are unanimous. It is more important even for politician-judges to decide many cases correctly than to agree with their co-partisans. Political identification matters as a proxy for how one's view affects how one evaluates a case. That is why judges do tend to vote with co-partisans. But this general tendency does not produce different independence outcomes in electoral and appointment systems because independence for all but a tiny number of cases is unobservable (and therefore not a focus of judges under either type of selection system).

In sum, a simple explanation for our results is that electoral judgeships attract and reward politically savvy people, whereas appointed judgeships attract more professionally able people (the selection argument). Elected judges try to decide a lot of cases because productivity is observable, whereas appointed judges write better opinions because they care more about their long-term reputation among professionals (the incentives/multitasking argument). It is possible that the politically savvy people might give the public what it wants—adequate rather than great opinions, issued in greater quantity and therefore (given the time constraint) greater average speed.

6. Conclusion

We began this project with the assumption that the data would demonstrate that appointed judges are better than elected judges. Our results suggest a more complicated story. It may be that elected judges are, indeed, superior to appointed judges. Or it may be that elected judges are superior to appointed judges *in small states only* and not necessarily in large states. At a minimum, the conventional wisdom needs to be reexamined.

A full comparative evaluation of the systems would require more research. There has been much concern in recent years about the rise in the cost of election campaigns for state supreme court justices, and some evidence that supreme court justices are more likely to vote in favor of contributors and their interests (Goldberg et al. 2006; Liptak and Roberts 2006).²² However, by the same token campaign expenditures enhance public awareness and indicate that judges face real political competition, which might be a good thing. In addition, appointment systems have hidden costs, namely, that they can serve patronage purposes. Empirical examination of the relationship between campaign expenditures and judicial quality is a promising avenue for future research.

22. Goldberg et al. (2006) identify Alabama, Illinois, Michigan, and Ohio as outliers—states where campaign contributions were much greater than in other states—as of 2000, the last year of our study. We checked the independence scores of judges in these states and found Alabama and Michigan had lower mean independence scores than the mean for all states, whereas Illinois and Ohio had higher scores. This is an interesting topic for future research, but as of now the data do not seem reliable and accessible enough to do a rigorous test.

Table A1. Key Variable Definitions

Variable	Definition
Total Opinions	Total number of majority, concurring, and dissenting opinions authored by a particular judge in 1 year (ranging from 1998 to 2000).
Outside State Citations	Total number of citations from (1) federal courts outside the circuit that includes the state in question and (2) courts in other states. Citations are measured in opinions authored up until January 1, 2007 (as tracked in the LEXIS Shepard's database).
Opposite_Party	The total number of opposing opinions written against an opposite party judge divided by the total number of opposing opinions written against either a judge of the opposite or same party as the judge in question for the 1998 to 2000 time period. Opposing opinions include dissents written against a majority opinion and majority opinions where a dissenting opinion exists.
Opposite_Pool	Total number of majority opinions written by the high court judges of the opposite political party (from the perspective of the judge in question) divided by the total number of majority opinions written by judges of both the same and opposite parties from 1998 to 2000.
Independence	Defined as Opposite_Pool minus Opposite_Party. A more negative Independence score occurs when Opposite_Pool < Opposite Party, indicating an increased tendency to write an opposing opinion against an opposite party judge. Conversely, a more positive Independence score indicates a decreased tendency to write an opposing opinion against an opposite party judge.
Election Non-Partisan	Indicator variable equal to 1 if the state uses a non-partisan election to select high court justices and 0 otherwise.
Election Partisan	Indicator variable equal to 1 if the state uses a partisan election to select high court justices and 0 otherwise.
Merit Plan	Indicator variable equal to 1 if the state follows the Missouri Merit Plan or a variant (including the Tennessee Plan) to select High Court justices and 0 otherwise.
Tenure	The average tenure of high court judges for the state in question, measured as of the spring of 1997 (from Hanssen 1999, table 1).
Number of Dissents	Indicator Variable equal to 1 if the judge authoring an opinion is Republican and 0 otherwise.
West Key Pages	Number of pages in an opinion associated with the West key pages section (as provided in the West reporter version of the opinion and tabulated on Westlaw).
Opinion Length	Number of pages from the start of the opinion to the end of the opinion as provided in the West reporter version of the opinion and tabulated on Westlaw. For majority opinions, we measured from where the authoring judge's actual opinion starts to the end of the majority opinion.
Open Secrets	Indicator Variable equal to 1 if the judge authoring the opinion in question has donated to a political candidate and 0 otherwise. Political contributions are tracked by www.opensecrets.org and include Federal Election Commission records of receipts from all individuals who contribute at least \$200 from 1992 to 2006.

Table A2. Judge-Level Variable Definitions

Variable	Definition
Chief Judge	For year-level data, indicator variable equal to 1 if the judge in question is the chief judge of the court in the year in question and 0 otherwise. For pooled data, indicator variable equal to 1 if the judge in question is the chief judge of the court for any year from 1998 to 2000 and 0 otherwise.
Court Experience	For year-level data, the difference between the year in question and the year the judge first joined the high court. For pooled data, the difference between 1998 and the year the judge first joined the high court (if the judge started on the court in 1998 or later court experience is set to 0).
Post-Law School Experience	The difference between 1998 and the year the judge graduated law school.
Retirement within ≤ 1 year	We calculate the number of years to retirement as equal to the year of retirement—the year in question. Indicator variable equal to 1 if the number of years to retirement is equal to 1 year or less and 0 otherwise. For pooled data, we calculate the number of years to retirement as equal to the year of retirement—2000.
Retirement in 2 years	We calculate the number of years to retirement as equal to the year of retirement—the year in question. Indicator variable equal to 1 if the number of years to retirement is equal to 2 years and 0 otherwise. For pooled data, we calculate the number of years to retirement as equal to the year of retirement—2000.
Retirement in 3 years	We calculate the number of years to retirement as equal to the year of retirement—the year in question. Indicator variable equal to 1 if the number of years to retirement is equal to 3 years and 0 otherwise. For pooled data, we calculate the number of years to retirement as equal to the year of retirement—2000.
Retirement in 4 years	We calculate the number of years to retirement as equal to the year of retirement—the year in question. Indicator variable equal to 1 if the number of years to retirement is equal to 4 years and 0 otherwise. For pooled data, we calculate the number of years to retirement as equal to the year of retirement—2000.
Age	Age of the judge in years.
Female	Indicator variable equal to 1 if the judge is female and 0 if male.
Private Practice	Indicator variable equal to 1 if the judge had private practice experience before becoming a judge and 0 otherwise.
Election Spending	Indicator variable equal to 1 if the judge raised funds relating to election campaign expenditures for the current year and 0 otherwise.
PAJID Score	PAJID score for each judge as developed by Brace et al. (2000). These scores locate judges on a political continuum from highly conservative (0) to highly liberal (100)

Table A3. Court-Level Variable Definitions

Variable	Definition
Adjusted Associate Justice Salary	For year-level data, the associate justice salary reported in the prior year for the state (so 1997 for 1998 judge-level data) divided by a cost of living adjustment for the year in question measured for the metro area in which the high court of the state is located. For pooled data, the associate justice salary reported in 1997 divided by the cost of living adjustment for 1998 (in thousands of dollars)
Adjusted Partner Salary	For year-level data, the average partner salary reported for the year in question for the state divided by a cost of living adjustment for the year in question measured for the metro area in which the high court of the state is located. For pooled data, the average partner salary in 1998 divided by the cost of living adjustment for 1998 (in thousands of dollars).
Stable Court	Indicator variable equal to 1 if the state high court justices stayed the same from 1998 to 2000 and 0 otherwise.
Number of Active Judges on Bench	Number of judges who were active at any time from 1998 to 2000 for the state in question.
No Mandatory Retirement	Indicator variable equal to 1 if the judges on the state high court do not face mandatory retirement and 0 otherwise.
Long-Term Clerk	Indicator variable equal to 1 if state clerks are tenured for more than one year and 0 if tenure is 1 year or less.
Number of Clerks Per Judge	Average number of clerks per judge in the 1998 to 2000 time period.
Law Clerk Opportunity Cost	The difference between the average salary of an entering associate at law firm in that state and the law clerk salary (in thousands of dollars).
Number of Trial Cases in the State	Number of trial cases in the entire state in 1998 (in thousands).
Intermediate Appellate Court	Indicator variable equal to 1 if the opinion is in opposition to the opinion of another judge in the same case and 0 otherwise. In the case of a dissenting opinion written by the judge in question, the opinion is treated as in active opposition to the majority opinion. In the case of a majority opinion by the judge in question, active opposition exists if the majority opinion is opposed by a dissenting opinion.
Mandatory Publication	Indicator variable equal to 1 if judges on the state high court face a mandatory publication rule and 0 otherwise.

Table A4. State-Level Variable Definitions

Variable	Definition
State Age	Age of the state. For year-level data this is defined as the difference between the year in question and the year of admission of the state into the United States. For pooled data, this is defined as the difference between 1998 and the year of admission of the state into the United States.
State Population	For year level data, the population of the state in millions measured in the year prior to the year in question (so the population in 1997 if the data year is 1998). For pooled data, the population of the state in millions measured for 1997.
Border Population	Total population of all bordering states of the state in question (measured as of 1997 in millions).
Crime Index	For year level data, overall crime rate for the state (including property and violent crime) per 100,000 people from the FBI Crime Report for the year prior to the year in question. For pooled data, the overall crime rate measured for 1997.
Gross State Product	Gross State Product (measured as of 1998 in billion of dollars).
Median Age of Population	Median age of state population (2000 US Census)
State Median Income	Median per capita income of the state population (2000 US Census in thousands of dollars)
Black Population Fraction	Fraction of the population comprised of blacks as obtained from the 2000 Census.
Citizen Ideology Score	Measure of citizen ideology based on election results in each district, which are then used to compute a state-wide average (ultimately based on interest group ratings of a given state's federal congressional delegation) (from Berry et al. 1998).
Law Enforcement Agencies	Number of law enforcement agencies in 2000 (from Reaves and Hickman 2000)
Law Enforcement Employees	Number of full-time law enforcement employees in 2000 (from Reaves and Hickman 2000)
Prosecutions	Number of prosecutions in 2000 (from Rottman and Strickland 2000)
Law-Related Employees	Number of law-related employees in 2000 (from Reaves & Hickman, 2000)
Law-Related Payroll	Annual payroll for all law-related employees in 2000 (from Reaves and Hickman 2000)
Law Establishments	Number of law firms (from Reaves and Hickman 2000)

Table A5. Subject Matter Categories

Variable	Definition
Administrative	Review of Agency/Government Decision making (not in another subject matter category). Also includes Government Actions (e.g., State suit to comply with state statute that does not fit in other categories); private actions suing state actors for negligence, etc (unless the case involves prisoner rights which is included in the "Criminal" category of cases).
Attorney and Client	Attorney Misconduct; Attorney fees (unless fits in one of above categories); Disbarment; contempt of court order against attorney
Capital Punishment	Capital Punishment-related actions.
Church and State	Pledge of Allegiance; Funding for Private Religious Schools; Prayer in School; Ten Commandments.
Commercial	Contracts; Insurance; Private arbitration; Creditor v. Debtor; Lessor-Lessee; Usury Laws; Franchise v. Franchisor; Employment Contractual Disputes; Corporate Law; Piercing the Corporate Veil; Tax; Bankruptcy; Enforcement of mechanics lien; Implied warrant of merchantability.
Criminal	Sentencing Guidelines; Prisoners Rights; Murder; Rape; Drugs/Controlled Substances; Attorney-Client Privilege in Criminal Context; Grand Jury-related; Juvenile Criminals. Excludes Capital Punishment cases.
Family	Divorce; Adoption; Child Support; Probate/Inheritance.
First Amendment	Employment issues (excluding employment contractual disputes); ERISA; National Labor Relations Board (NLRB); Occupational Safety and Health Act (OSHA); Fair Labor Standards Act (FLSA); Wrongful Discharge; Labor Management Relations Act (LMRA); Family and Medical Leave Act (FMLA); Employee Benefits; Worker's Compensation claims; Retaliatory Discharge claims.
Labor	Employment issues (excluding (1) employment contractual disputes that are not Workers Comp or state administrative wage rate related—these go to "Commercial" and (2) excluding discrimination-type claims that fit in "Civil Rights"); ERISA; NLRB; Occupational Safety and Health Act (OSHA); Fair Labor Standards Act (FLSA); Wrongful Discharge; Labor Management Relations Act (LMRA); Family and Medical Leave Act (FMLA); Employee Benefits; Worker's Compensation claims; Retaliatory Discharge claims; State Wage Rate Claims
Property	Takings claims; Zoning issues; Property rights; Property Licensing-Related or Permit-Related; Landlord-Tenant-Related.
Rights	Race Discrimination; Sex Discrimination; Affirmative Action; Civil Rights; Age Discrimination; Privacy; Handicap Discrimination; Abortion (Includes discrimination in employment context cases); Voting Rights-Voting Related
Torts	Federal Tort Related Act; Medical Malpractice; Products Liability; Wrongful Death; Libel; etc.
Other	All other cases.

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